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*THE PHYSICAL PROPERTIES OF AQUEOUS SALT SOLUTIONS IN RELATION TO THE IONIC THEORY.**

IT is generally recognized that the further progress of physical science will be greatly facilitated by a better systematization of the knowledge already accumulated, and this is true in an especially high degree of the newly developed branch of science in which this section is directly interested. It has, therefore, seemed to me that the most valuable contribution that I could make toward the solution of the present problems of physical chemistry in correspondence with the aims of this Congress, would be a formulation of the present status of some of our knowledge relating to important classes of phenomena which are being actively investigated, but which have not yet received a final interpretation. It was my original hope to discuss several such classes of phenomena; but the effort involved in the collation and criticism of the available data connected with the problem which was first studied, forced me to confine my attention to that alone. This problem concerns *the physical properties of aqueous salt solutions in relation to the ionic theory.* This is the subject which I shall attempt to present to you: I hope that its importance and the greater definiteness that can be given to its treatment may compensate for the somewhat limited scope of this paper.

Permit me to say in advance that I have

* Address delivered at St. Louis before the Section of Physical Chemistry of the Congress of Arts and Science.

studied this subject primarily from an empirical standpoint, and that it will be my aim to present to you a series of generalized statements of the experimental results, formulated in such a way as to show their relation to the important hypotheses connected with the ionic theory. Unfortunately, it will not be possible in this address to reproduce, or even fully refer to, the data upon which these conclusions are based—a defect serious in a work of this kind, which will be remedied in a subsequent publication. I shall, however, try to show the general character of the evidence for each conclusion, and the degree of accuracy within which it has been confirmed. I wish to add that I have been most ably assisted in the preparation of the material upon which paper is based, by Dr. J. W. Brown and Dr. M. S. Sherrill, of the Massachusetts Institute of Technology.

The principles to be first presented have reference to two of the main hypotheses which are commonly employed in quantitative applications of the ionic theory. One of these hypotheses is that *the migration-velocities of the ions of a salt do not vary appreciably with its concentration, at least up to a moderate concentration; and consequently, that the degree of ionization is equal to the ratio of the equivalent conductivity at the concentration in question to the limiting value of the equivalent conductivity at zero concentration*—a ratio which I will hereafter call simply the conductivity-ratio. The other hypothesis is that *ions, and also the un-ionized molecules accompanying them, produce an osmotic pressure substantially equal to the pressure exerted by the same number of gaseous molecules at the same temperature, at least up to a moderate concentration;* a hypothesis which may be more briefly expressed by the statement that the osmotic-pressure constant for dissolved electrolytes is identical with the gas-constant. It is evident that with the help of this hypothesis we can calculate, either from measurements of osmotic pressure or from those of any other property which is thermodynamically related to osmotic pressure, the number of mols in the solution resulting from one formula weight of salt, that is, the quantity which van't Hoff has represented by the letter *i*. From the latter, provided the ionization is not complicated by the formation of complex molecules or ions, the degree of ionization is readily derived.

The first of these hypotheses can not be independently tested, because no direct method of determining the change of migration-velocity with the concentration, is known. But the following principle, which has an important significance with reference to the *relative* influence of concentration on the velocities of different ions, has been established by measurements of the concentration-changes at the electrodes attending the electrolysis of salt solutions.

The transferrence number, or ratio of the conductivity of one ion to the sum of the conductivities of both ions, is constant within one per cent., between the concentrations of 1/200 *and* 1/10 *normal, for all salts thus far accurately investigated, except lithium chloride, the halides of bivalent metals and cadmium sulphate.*

This principle holds true, according to the results of various investigators, in the case of potassium and sodium chlorides, hydrochloric and nitric acids, silver nitrate, barium nitrate, potassium sulphate and copper sulphate—thus in the case of salts of the three different ionic types, which I will speak of as the uni-univalent, the uni-bivalent and the bi-bivalent types, in correspondence with the valences of the two ions composing the salt.

Two conclusions are to be drawn from this result. The first is that complex ions are not present in important quantity in

the solutions of these salts. And the second is that the migration velocities of the two ions of a salt vary by the same percentage amount, if they vary at all, with changes in its concentration. It is scarcely admissible, however, to regard this last fact even as an indication that the hypothesis of constant migration velocities is correct; for any change in the character of the liquid medium might well affect the velocities of different ions not far from equally.

Important evidence in regard to this hypothesis and that stating that ions and the un-ionized molecules associated with them have a normal osmotic pressure is, however, furnished by the agreement of the ionization values derived, on the one hand, from the conductivity-ratio, and, on the other, from the properties thermodynamically related to osmotic pressure. Three of these properties have been measured with sufficient accuracy with certain electrolytes to make the results of significance, namely, the freezing-point lowering, the electromotive force of concentration cells, and the heat of solution in relation to change of solubility with the temperature. Under the assumption that osmotic pressure and gaseous pressure are equal under identical conditions, a relation between each of these properties and the degree of ionization of an electrolyte can be derived with the help of the second law of energetics. Then, either this ionization value may be directly compared with the conductivity ratio; or, assuming provisionally that the latter is a correct measure of ionization, the magnitude of the property in question may be calculated, and the result compared with that obtained by direct measurement. In the case of the freezing-point lowering, I have adopted the first of these methods. For the five salts for which both reliable freezing-point determinations and accurate conductivity measurements at 0° exist, the ionization values corresponding to both of these properties have been computed. Especial attention was given to the selection of the best value of the freezing-point lowering constant and to the extrapolation of the conductivity for zero concentration, the details of which can not be here described. The results may be summarized as follows:

In case of the two uni-univalent salts and the three uni-bivalent salts hitherto carefully investigated, the ionization values derived from freezing-point lowering do not differ from those derived from conductivity, between the concentrations of 1/200 *and* 1/4 *normal, by more than two or three per cent.*

The five salts referred to are potassium and sodium chlorides, potassium and sodium sulphates and barium chloride. The two sets of values for potassium chloride, for which an abundant experimental material exists, exhibit no pronounced or systematic differences; but for the other four salts the freezing point leads to values which are in general from two to three per cent. higher at all concentrations than the conductivity ratio. The fact that these differences do not, as a rule, increase with increasing concentration indicates that they may be due to some constant experimental error, or to an error in the extrapolated conductivity value.

Accurate measurements have been made by Jahn of the electromotive force of concentration cells consisting of two silver or mercury electrodes covered with silver chloride or mercurous chloride, one of which is immersed in a weak solution and the other in a strong solution of sodium or potassium chloride. These measured values were compared by him with those calculated from the thermodynamic relation between electromotive force and the concentrations and degrees of ionization of the salt in the cell. Unfortunately, however, the thermodynamic relation employed in-

volved the assumption that the ionization varies with the concentration in accordance with the mass-action law—an assumption which is known not to be true of the ionization values derived from conductivity. The assumption is, therefore, an irrational one—one by which the question at issue is prejudged. What should be done in calculating the electromotive force so as to determine whether the conductivity ratio gives ionization values consistent with the measured electromotive forces, is evidently to assume that the ionization changes with the concentration in the way that the conductivity indicates that it does. Arrhenius recognized this error and partially corrected for it by a method of approximation. I have repeated the calculations by an exact thermodynamic formula based on an empirical law expressing the change of the conductivity ratio with the concentration, to which I will refer later. The results are summed up in the statement that, *when the conductivity-ratio is assumed to represent the degree of ionization of the salt, the calculated values of the electromotive force of concentration cells exceed the measured ones by only about one per cent. in the case of potassium and sodium chloride between the concentrations of* 1/600 *and* 1/20 *normal.* The measured electromotive force corresponds to an ionization value at the latter concentration about one per cent. less than the conductivity ratio.

The thermodynamic relation involving heat of solution has been accurately tested with only one salt—potassium perchlorate; but since it is a different salt from those used in the other experiments and since its concentration was fairly high—one eighth normal—the result is of interest. It was found that the *measured heat of solution was less by only 1.1 per cent. than that calculated under the assumption that the conductivity ratio is equal to the degree of ionization.* The measured heat of solution corresponds to an ionization value $2\frac{1}{2}$ per cent. lower than the conductivity ratio.

With respect to these small deviations of the results obtained by the three methods of comparison, it is important to note that they lie in opposite directions, the freezing-point lowering corresponding to larger values of the ionization, and the measured electromotive forces and heat of solution to smaller ones than the conductivity-ratio. This fact makes it almost certain that they are due to experimental errors. Nevertheless, further exact measurements of all these properties are highly desirable.

From a theoretical standpoint these three methods are based on the same hypotheses, namely, that the osmotic pressure constant for ions and un-ionized molecules is identical with the gas-constant; that the conductivity-ratio is a correct measure of ionization, and that complex molecules or ions are not present in the solution. The concordance of the results furnishes, therefore, a strong confirmation of the correctness of these fundamental hypotheses. The only alternative conclusion is that an error in one of these hypotheses is compensated by an error of opposite effect in one of the others; but it seems very improbable that such a compensation could occur in the case of so many salts of different chemical nature and different types through the range of concentration (1/200–1/4 normal) for which the agreement of the experimental results has been shown to hold true. It is certainly more consistent with the modern methods of science to adopt these simpler hypotheses which are in full accord with the considerable number of facts thus far known, than deliberately to introduce more complicated assumptions for which there is at present no experimental warrant.

The combination of these hypotheses with the experimental values of the quantities involved at varying concentrations makes

necessary the further conclusion that the *degree of ionization of salts, whether derived from the conductivity-ratio or from thermodynamic relations involving the equality of the osmotic pressure constant and the gas constant does not vary with the concentration even approximately in accordance with the law of chemical mass action.*

This empirical consequence of the fundamental hypotheses of the ionic theory has led several investigators to raise a theoretical objection to them, it being contended that the laws of thermodynamics require that the validity of these hypotheses involves that of the mass action law itself. This apparent inconsistency between the inductive and deductive conclusions makes it probable that some unproved, erroneous assumption is tacitly involved in the theoretical derivation. That there is in fact a possible alternative, which has, I believe, been previously overlooked in the thermodynamic discussions, will be evident from the following considerations. The thermodynamic relations between ionization and freezing point, electromotive force, or heat of solution involve only the assumption that the work done in reversibly separating water from a solution at constant concentration is equal to that done in producing the same volume-change in a gas, which implies, of course, that the ions and un-ionized molecules have in the presence of each other normal osmotic pressures. On the other hand, the derivation of the mass action law equation is based on cyclical processes which necessarily involve the separate introduction and removal of the un-ionized molecules and of the ions into or from solutions of different concentrations, and it further involves the assumption that this introduction or removal of molecules or ions can be effected by the application of an external pressure equal to that osmotic pressure which each of them possesses in the mixture, that is, the possibility is ignored that the separation of the molecules from the ions may itself give rise to some new force, and may involve, consequently another quantity of work than that corresponding to the osmotic pressure. The ionic theory would evidently predict a result of this kind if an attempt were made to separate the positive ions from the negative, even though their osmotic pressures when present together were perfectly normal; and it is quite conceivable, even though the reason for it be not apparent, that the separation of the un-ionized molecules from the ions, with which they may be in electrical as well as chemical equilibrium, should involve an abnormal quantity of work. The assertion that the validity of the osmotic pressure principle necessarily implies that of the mass action law is, therefore, unwarranted from a deductive standpoint; while the inductive evidence, pointing strongly as it does to the substantial correctness of the former principle and the complete inadequacy of the latter one, makes it highly probable that the *separation of un-ionized molecules from ions does involve the expenditure of other work than that corresponding to their osmotic pressures.*

Since the ionization does not change with the concentration in accordance with the mass action law, it is natural to inquire what the law of its change is. This matter has been investigated from an empirical standpoint by several investigators with the help of the conductivity data. The results justify the statement of the following principles:

The un-ionized fraction of a salt as determined from the conductivity ratio is proportional to the cube-root of its total concentration or to that of its ion-concentration, between 1/2,000 *and* 1/10 *normal, in the case of both uni-univalent and uni-multivalent salts.* That is, $1 - \gamma = Kc^{\frac{1}{3}}$

or $1-\gamma = K(c\gamma)^{\frac{1}{2}}$ where γ is the degree of ionization, c the concentration, and K a constant. The first of three functions was proposed by Kohlrausch, the second by Barmwater. Owing to the relatively small variation of the ionization, these two functions can not differ much as to their constancy, but on the whole the experimental data indicate that the second function is somewhat more constant. The average deviations of the actual measurements from the values corresponding to this function are $\frac{1}{4}$ per cent. in the case of ten uni-univalent salts, $\frac{1}{3}$ per cent. in the case of nine uni-bivalent salts and also $\frac{1}{3}$ per cent. in the case of three uni-tri and uni-quadrivalent salts. The maximum deviations are two or three times as great. It is of interest to note that the strong mineral acids, hydrochloric and nitric, behave like salts in this respect. These functions have been shown to apply to potassium and sodium chlorides through a range of temperature extending from 18° to 306°. They do not apply at all closely to such salts of the bi-bivalent type as magnesium and copper sulphates, perhaps owing to appreciable hydrolysis. Nor do they represent satisfactorily the experimental data for any kind of salts at the very low concentrations lying between 1/100,000 and 1/2,000 normal, nor at concentrations higher than 1/5 normal.

The experimental results are also well expressed by the statement that *in the case both of uni-univalent and uni-bivalent salts between the concentrations of* 1/10,000 *and* 1/5 *normal, the concentration of the un-ionized molecules is proportional to the concentration of the ions raised to a constant power, varying somewhat with the salt and the temperature, but as a rule only between the limits of* 1.43 *and* 1.56. That is,

$$c(1-\gamma) = K(c\gamma)^n, \text{ where } n > 1.43 \text{ and } < 1.56.$$

This general function was first applied by Storch, and was afterwards further discussed by Euler and Bancroft. It has the advantage over the previous ones that it represents the data with accuracy even up to the highest dilutions, and therefore can be used for obtaining the limiting conductivity at zero concentration.

The applicability to the salts of different types of either of these principles governing the change of ionization with the concentration leads to the important conclusion that *the form of the concentration function is independent of the number of ions into which the molecules of the salt dissociates.* This remarkable fact, though previously recognized, has not been sufficiently emphasized, and it has been often ignored in discussions of the cause of the deviation of the ionization of salts from the requirements of the mass action law. It seems to me to show almost conclusively that chemical mass action has no appreciable influence in determining the equilibrium between ions and un-ionized molecules. How complete the contradiction with the mass action law is, may be illustrated by citing the specific facts that for di-ionic, tri-ionic and tetra-ionic salts this law requires that the concentration of the un-ionized molecules be proportional to the square, the cube, and the fourth power, respectively, of the concentration of the ions, while the experimental data show that it is approximately proportional to the 3/2 power of that concentration, whatever may be the type of salt.

Having seen in what manner the degree of ionization varies when the concentrations of both ions of the salt are simultaneously varied by dilution, it is of interest to determine the effect of changing the concentration of either ion separately. A study of the conductivity and the freezing-point of mixtures of two salts having one ion in common throws much light upon

this question, for the following simple principle has been found to represent this phenomenon: *The conductivity and the freezing-point lowering of a mixture of salts having one ion in common are those calculated under the assumption that the degree of ionization of each salt is that which it would have if present alone at such an equivalent concentration that the concentration of either of its ions were equal to the sum of the equivalent concentrations of all the positive or negative ions present in the mixture.*

This somewhat complicated statement may be illustrated by the following example: Suppose that a mixed solution is 0.1 normal with respect to sodium chloride and 0.2 normal with respect to sodium sulphate, and that it is 0.18 normal with reference to the positive or negative ions of these salts. The principle then requires that the ionization of either of these salts in the mixture be the same as it is in water alone when its ion-concentration is 0.18 normal.

This principle in regard to the conductivity of mixtures, which has been definitely stated by Arrhenius, is shown by the existing data to hold true almost, if not quite, within the small experimental error of the determinations both for mixtures of salts of the same type and for those of salts of different types up to a concentration of at least 1/2 normal. Experiments confirming this principle have been made upon eight pairs of uni-univalent salts by Arrhenius, Manson and Barmwater. In addition, the principle has been shown by several Canadian investigators, Archibald, McKay and Barnes, to hold true for mixtures of potassium and sodium sulphates, potassium and copper or magnesium sulphates (up to 0.1 normal), potassium sulphate and chloride, barium and sodium chlorides, and zinc and copper sulphates—thus for almost every possible typical combination of uni-, uni-bi and bi-bivalent salts. That the same principle is true of the freezing point lowering is shown by the measurements of Archibald with mixtures of potassium and sodium sulphate. This proves that the phenomenon really has reference to the degree of ionization and that it does not arise from a possible variation in the migration-velocities of the ions.

Of special interest is the relation of this principle to the validity of the mass action law. Almost all investigators of the conductivity of mixtures have concluded from the fact that upon mixing solutions of equal ion-concentration there is no change in ionization, that the results do conform to this law. Yet it is scarcely conceivable that this law can apply to mixtures of salts in which the concentration of one ion is varied while maintaining that of the other constant, in view of the fact that it is known not to hold true for the variations of the concentrations of both ions produced by dilution. And in reality this conclusion, if regarded as a general expression of the facts, is entirely unwarranted. It is true that for certain typical combinations of salts—those for which from one molecule of each salt results by ionization not more than one ion of the kind not common to the salts—the principle here stated does coincide with the requirements of the mass-action law. But for combinations not so characterized the mass-action law predicts, as is readily seen upon formulating the equations, a conductivity of the mixture widely divergent from that actually found, and, therefore, from that expressed by the principle under consideration. This last statement applies, for example, to the mixtures before referred to of potassium sulphate with sodium sulphate, and of potassium sulphate with copper or magnesium sulphate, the first of which have been studied with respect both to their conductivity and freezing point.

The law of chemical mass action here again shows itself entirely inapplicable to the phenomena connected with the ionization of salts. That some investigators have thought that the deviations from this law indicated by the conductivity were only apparent, and that they were attributable to variations in the migration velocity, has arisen, no doubt, from the fact that they have confined their attention to di-ionic salts, and have failed to recognize, on the one hand, the striking divergences from it exhibited by tri-ionic salts, and, on the other, the substantial correspondence of the conductivity and freezing-point results.

Combining this principle in regard to the ionization of mixed salts in solution with the empirical concentration law of Storch for single salts, we are led to the conclusion that the ratio of the concentration of the un-ionized part to the product of the concentrations of the two ions (but in the case of tri-ionic salts *not* raised to a power corresponding to the requirements of the mass action law) is a function of the sum of the equivalent concentrations of all the ions in the solution and of that alone.* This ratio is, moreover, roughly inversely proportional to the square root of the total ion-concentration.

The correctness of this principle is further demonstrated by the fact that with its aid, also, the conductivity of a mixture of two salts without a common ion can be computed from their separate conductivities. This is shown by the conductivity measurements, made by Archibald and more recently by Sherrill, upon mixtures of potassium chloride and sodium sulphate, or of sodium chloride and potassium sulphate. Up to at least 0.2 normal concentration, the agreement between the observed and calculated values is within 0.5 per cent. On the other hand, the divergence of the observed values from the requirement of the mass-action law amounts to many per cent.

It seems appropriate at once to supplement these principles in regard to the form of the concentration function by a statement of two general rules which have been found to express the magnitude of the ionization of salts of different types. These rules, unlike the preceding principles, are only crude approximations; but, nevertheless, they prove of some assistance in rough applications of the ionic theory, and undoubtedly possess an important theoretical significance, not yet recognized. They may be stated as follows: (1) *The decrease of ionization with increasing concentration is roughly constant in the case of different salts of the same type; and* (2) *the un-ionized fraction at any definite molal concentration is roughly proportional to the product of the valences of the two ions in the case of salts of different types.* Thus, at 0.1 normal concentration the mean value of the degree of ionization for 17 uni-univalent salts measured at 18° is 83.3 per cent., the average deviation of the separate values from this mean is 2.1 per cent., and the maximum deviation of any of them is 5.4 per cent. of the mean value; while for fourteen uni-bivalent salts the mean value is 69.8 per cent., the average deviation 5 per cent. of this, and the maximum deviation about 10 per cent. of it. The un-ionized fraction in 1/20 molal solution is 13½ per cent. for these univalent salts; 30 per cent., or about twice as great, for the uni-bivalent salts; and 60 per cent., or about four times as great, for the three bi-bivalent salts investigated (zinc, magnesium and copper sulphates). The salts of mercury

* This is expressed mathematically by the following equation, in which c_1 and c_2 represent the equivalent concentrations of the two salts, and γ_1 and γ_2 their degrees of ionization in the presence of each other:

$$\frac{c_1\gamma_1(c_1\gamma_1 + c_2\gamma_2)}{c_1(1-\gamma_1)} = K_1(c_1\gamma_1 + c_2\gamma_2)^{n-1}$$

and cadmium are pronounced exceptions to the rule.

A far more extensive material for testing these rules is furnished by the measurements made at 25° between the concentrations of 1/32 and 1/1024 normal. In the case of the uni-univalent salts, data exist at this temperature and these concentrations for 36 inorganic salts, about 65 sodium salts of organic acids, and about an equal number of hydrochlorates of organic bases. A consideration of all these data shows that, with only three or four exceptions not of a pronounced character, the values of the degree of ionization of all these salts in 1/32 normal solution lie between the limits of 84 and 90 per cent. and are fairly uniformly distributed throughout this range of 6 per cent. For 67 uni-bivalent salts the corresponding limits of the ionization values are 72 and 81 per cent. while for only four such salts do the values lie beyond these limits. For the 6 uni-trivalent salts investigated the range is from 67 to 76 per cent.; for the three uni-quadrivalent salts, from 59 to 63 per cent.; and for twelve bi-bivalent salts, from 49 to 63 per cent., while three such salts show more considerable variations. The values of the un-ionized fraction corresponding to the mean of these two limits for the different types of salts at the same equivalent concentration increase somewhat more slowly than the product of the valences of the ions. The proportionality becomes a fairly close one, however, when the salts are compared at the same molal, instead of the same equivalent concentration. Thus, with the help of the Kohlrausch concentration-function, it is calculated from the preceding values that the un-ionized fractions in 1/32 molal solution are as follows: 13 per cent. for the uni-univalent salts, 29½ per cent. for the uni-bivalent salts, 41 per cent. for the uni-trivalent salts, 62 per cent. for the uni-quadrivalent salts, 55 per cent. for the bi-bivalent salts, which are seen to be approximately the required multiples of the constant factor 14.

Before leaving this subject it should be stated that the results conform, on the whole, about equally well with the rule that *the decrease of equivalent conductivity (instead of ionization) is roughly constant for salts of the same type;* and when the comparison is made at the same equivalent concentration, distinctly better to the rule that the *decrease of equivalent conductivity is proportional to the product of the valences of the ions for salts of different types.* (When compared at the same *molal* concentration, however, this rule does not apply.) These rules were originally stated by Ostwald. They differ not inconsiderably from those expressing the change in ionization, namely, to an extent corresponding to the variations of the conductivities at extreme dilution. The deviations are so irregular, however, that from an empirical standpoint the choice between the two pairs of rules is arbitrary. In either form these rules seem to justify the inference that the degree of ionization of salts, unlike that of the organic acids and bases, is not primarily a specific chemical property determined by chemical affinity, but that it is determined, at least in the main, by the magnitude of the electric charges on the ions.

The establishment of the principle in regard to the ionization of a mixture of salts has a direct bearing on the phenomenon of the effect of one salt on the solubility of another with a common ion. It has been usually assumed that in a (not too concentrated) saturated solution the un-ionized molecules of the salt always have the same concentration, and secondly that the product of the *ion*-concentrations (each raised to a power corresponding to the number resulting from one molecule) also retains the same value. And the experimental results

in several cases have been shown to accord fairly well with these two hypotheses. Yet their simultaneous validity is quite inconsistent with the principle in regard to the ionization in mixtures. In fact, when considered in the light of this principle, the existing data lead to the conclusion that the former hypothesis is not even approximately true, and that the latter one, at any rate in cases where the ionization is far from complete, is affected by a considerable error. One example may be cited: when thallium chloride and bromate, each of which alone has a solubility of about 1/40 normal in water at 40°, are simultaneously present as solid phases, the solubility of each is reduced by the other to an extent which shows that the concentration of the un-ionized molecules is diminished by about 15 per cent. and that the product of the ion-concentrations is increased by about 5 per cent. This case is a typical one; but what the quantitative law of the influence in question is, can only be determined by a further study of the phenomenon. In the case of tri-ionic salts, the ion-concentration product is even approximately constant, only when the square—not when the first power—of the concentration of the univalent ion is employed. This has been shown by experiments with lead iodide in the presence of potassium iodide, with lead chloride in that of other chlorides and with calcium hydroxide in that of ammonium chloride.

I will close by calling your attention to a remarkable principle in regard to the properties of salt solutions, of a character quite distinct from those thus far considered. That many properties of dilute salt solutions can be expressed as the sum of values assigned once for all to the constituent radicals or ions was long ago recognized, and has often been cited as a corollary from the ionic theory. That this additivity of properties persists up to fairly high concentrations is a fact, however, that has received scant consideration, owing to its apparent lack of relationship to that theory. This fact is shown strikingly in the case of certain highly specific optical properties which are ordinarily found to be dependent in a high degree on molecular structure. Thus, the experimental data fully warrant the statement of the principle that *the optical activity and the color of salts in solution, when referred to equivalent quantities, are independent of the concentration and therefore of the degree of ionization of the salts and are additive with respect to the properties of the constituent ions even up to concentrations where a large proportion of the salt is in the un-ionized state.* Abundant data might be cited in support of this principle, especially with reference to optical activity. But I can only illustrate the character of the evidence by presenting a few of the results obtained by Walden with the salts of α-brom camphor sulphonic acid. In 1/30 normal solution he found the following values of the molal rotary power:

Lithium salt	275
Sodium salt	272
Potassium salt	273
Thallium salt	273
Acid itself	273
Beryllium salt	274
Zinc salt	272
Barium salt	272

The values are seen to be substantially identical, although the conductivity shows the acid to have an un-ionized fraction of 7 per cent., the salts of the univalent metals one of 16 per cent., and those of the bivalent metals one of 30 per cent., and although the un-ionized molecules present contain in some cases the elements hydrogen, lithium and beryllium of very small atomic weights, and in two others the elements thallium and barium of large atomic weights.

If there were not other evidence to the contrary, the existence of this general principle, which is also applicable to many other properties, would almost warrant the conclusion that the salts are completely ionized up to the concentration in question, and that the decrease in conductivity is due merely to a change in migration velocity. But, in view of the apparently conclusive evidence against such a hypothesis, we can only conclude that the form of union represented by the un-ionized molecules of salts differs essentially from ordinary chemical combination, it being so much less intimate that the ions still exhibit their characteristic properties, in so far as these are not dependent upon their existence as separate aggregates.

These then are the empirical principles to which a critical analysis of the experimental data leads. Upon these principles must be based the rational, theoretical explanation of the phenomena in question. The discovery of that explanation constitutes one of the most important of the present problems of physical chemistry.

ARTHUR A. NOYES.

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*THE FIELD OF LOGIC.**

CURRENT tendencies in logical theory make a determination of the field of logic fundamental to any statement of the general problems of the science. In view of this fact, I propose in this paper to attempt such a determination by a general discussion of the relation of logic to mathematics, psychology and biology, especially noting in connection with biology the tendency known as pragmatism. In conclusion, I shall indicate what the resulting general problems appear to be.

* Address before the Section of Logic, International Congress of Arts and Science, Universal Exposition, St. Louis.

I.

There may appear, at first, little to distinguish mathematics in its most abstract, formal and symbolic type from logic. Indeed, mathematics as the universal method of all knowledge has been the ideal of many philosophers, and its right to be such has been claimed of late with renewed force. The recent notable advances in the science have done much to make this claim plausible. A logician, a non-mathematical one, might be tempted to say, that in so far as mathematics is the method of thought in general, it has ceased to be mathematics; but, I suppose, one ought not to quarrel too much with a definition, but should let mathematics mean knowledge simply, if the mathematicians wish it. I shall not, therefore, enter the controversy regarding the proper limits of mathematical inquiry. I wish to note, however, a tendency in the identification of logic and mathematics which seems to me to be inconsistent with the real significance of knowledge. I refer to the exaltation of the freedom of thought in the construction of conceptions, definitions and hypotheses.

The assertion that mathematics is a 'pure' science is often taken to mean that it is in no way dependent on experience in the construction of its basal concepts. The space with which geometry deals may be Euclidean or not, as we please; it may be the real space of experience or not; the properties of it and the conclusions reached about it may hold in the real world or they may not; for the mind is free to construct its conception and definition of space in accordance with its own aims. Whether geometry is to be ultimately a science of this type must be left, I suppose, for the mathematicians to decide. A logician may suggest, however, that the propriety of calling all these conceptions 'space' is not as clear as it ought to be. Still further, there

seems to underlie all arbitrary spaces, as their foundation, a good deal of the solid material of empirical knowledge, gained by human beings through contact with an environing world, the environing character of which seems to be quite independent of the freedom of their thought. However that may be, it is evident, I think, that the generalization of the principle involved in this idea of the freedom of thought in framing its conception of space, would, if extended to logic, give us a science of knowledge which would have no necessary relation to the real things of experience, although these are the things with which all concrete knowledge is most evidently concerned. It would inform us about the conclusions which necessarily follow from accepted conceptions, but it could not inform us in any way about the real truth of these conclusions. It would, thus, always leave a gap between our knowledge and its objects which logic itself would be quite impotent to close. Truth would thus become an entirely extra-logical matter. So far as the science of knowledge is concerned, it would be an accident if knowledge fitted the world to which it refers. Such a conception of the science of knowledge is not the property of a few mathematicians exclusively, although they have, perhaps, done more than others to give it its present revived vitality. It is the classic doctrine that logic is the science of thought as thought, meaning thereby thought in independence of any specific object whatever.

In regard to this doctrine, I would not even admit that such a science of knowledge is possible. You can not, by a process of generalization or free construction, rid thought of connection with objects; and there is no such thing as a general content or as content-in-general. Generalization simply reduces the richness of content and, consequently, of implication. It deals with concrete subject-matter as much and as directly as if the content were individual and specialized. 'Things equal to the same thing are equal to each other,' is a truth, not about thought, but about things. The conclusions about a fourth dimension follow, not from the fact that we have thought of one, but from the conception about it which we have framed. Neither generalization nor free construction can reveal the operations of thought in transcendental independence.

It may be urged, however, that nothing of this sort was ever claimed. The bondage of thought to content must be admitted, but generalization and free construction, just because they give us the power to vary conditions as we please, give us thinking in a relative independence of content, and thus show us how thought operates irrespective of, although not independent of, its content. The binomial theorem operates irrespective of the values substituted for its symbols. But I can find no gain in this restatement of the position. It is true, in a sense, that we may determine the way thought operates irrespective of any specific content by the processes of generalization and free construction; but it is important to know in what sense. Can we claim that such irrespective operation means that we have discovered certain logical constants, which now stand out as the distinctive tools of thought? Or does it rather mean that this process of varying the content of thought as we please reveals certain real constants, certain ultimate characters of reality, which no amount of generalization or free construction can possibly alter? The second alternative seems to me to be the correct one. Whether it is or not may be left here undecided. What I wish to emphasize, is the fact that the decision is one of the things of vital interest for logic, and properly belongs in that science. Clearly, we can never know

the significance of ultimate constants for our thinking until we know what their real character is. To determine that character we must most certainly pass out of the realm of generalization and free construction; logic must become other than simply mathematical or symbolic.

There is another sense in which the determination of the operations of thought irrespective of its specific content is interpreted in connection with the exaltation of generalization and free construction. Knowledge, it is said, is solely a matter of implication, and logic, therefore, is the science of implication simply. If this is so, it would appear possible to develop the whole doctrine of implication by the use of symbols, and thus free the doctrine from dependence on the question as to how far these symbols were themselves related to the real things of the world. If, for instance, *a* implies *b*, then, if *a* is true, *b* is true, and this quite irrespective of the real truth of *a* or *b*. It is to be urged, however, in opposition to this view, that knowledge is concerned ultimately only with the real truth of *a* and *b*, and that the implication is of no significance whatever apart from this truth. There is no virtue in the mere implication. Still further, the supposition that there can be a doctrine of implication simply, seems to be based on a misconception. For even so-called formal implication gets its significance only on the supposed truth of the terms with which it deals. We suppose that *a does* imply *b*, and that *a is* true. In other words, we can state this law of implication only as we first have valid instances of it given in specific, concrete cases. The law is a generalization and nothing more. The formal statement gives only an apparent freedom from experience. Moreover, there is no reason for saying that *a* implies *b* unless it does so either really or by supposition. If *a* really implies *b*, then the implication is clearly not a matter of thinking it; and to suppose the implication is to feign a reality, the implications of which are equally free from the processes by which they are thought. Ultimately, therefore, logic must take account of real implications. We can not avoid this through the use of a symbolism which virtually implies them. Implication can have a logical character only because it has first a metaphysical one.

The supposition underlying the conception of logic I have been examining is, itself, open to doubt and seriously questioned. That supposition was the so-called freedom of thought. The argument has already shown that there is certainly a very definite limit to this freedom, even when logic is conceived in a very abstract and formal way. The processes of knowledge are bound up with their contents, and have their character largely determined thereby. When, moreover, we view knowledge in its genesis, when we take into consideration the contributions which psychology and biology have made to our general view of what knowledge is, we seem forced to conclude that the conceptions which we frame are very far from being our own free creations. They have, on the contrary, been laboriously worked out through the same processes of successful adaptation which have resulted in other products. Knowledge has grown up in connection with the unfolding processes of reality, and has, by no means, freely played over its surface. That is why even the most abstract of all mathematics is yet grounded in the evolution of human experience.

In the remaining parts of this paper, I shall discuss further the claims of psychology and biology. The conclusion I would draw here is, that the field of logic can not be restricted to a realm where the operations of thought are supposed to move freely, independent or irrespective of their contents and the objects of a real world;

and that mathematics, instead of giving us any support for the supposition that it can, carries us, by the processes of symbolization and formal implication to recognize that logic must ultimately find its field where implications are real, independent of the processes by which they are thought, and irrespective of the conceptions we choose to frame.

II.

The processes involved in the acquisition and systematization of knowledge may, undoubtedly, be regarded as mental processes and fall thus within the province of psychology. It may be claimed, therefore, that every logical process is also a psychological one. The important question is, however, is it nothing more? Do its logical and psychological characters simply coincide? Or, to put the question in still another form, as a psychological process simply, does it also serve as a logical one? The answers to these questions can be determined only by first noting what psychology can say about it as a mental process.

In the first place, psychology can analyze it, and so determine its elements and their connections. It can thus distinguish it from all other mental processes by pointing out its unique elements or their unique and characteristic connection. No one will deny that a judgment is different from an emotion, or that an act of reasoning is different from a volition; and no one will claim that these differences are entirely beyond the psychologist's power to ascertain accurately and precisely. Still further, it appears possible for him to determine with the same accuracy and precision, the distinction in content and connection between processes which are true and those which are false. For, as mental processes, it is natural to suppose that they contain distinct differences of character which are ascertainable. The states of mind called belief, certainty, conviction, correctness, truth are, thus, doubtless all distinguishable as mental states. It may be admitted, therefore, that there can be a thoroughgoing psychology of logical processes.

Yet it is quite evident to me that the characterization of a mental process as logical is not a psychological characterization. In fact, I think it may be claimed that the characterization of any mental process in a specific way, say as an emotion, is extra-psychological. Judgments and inferences are, in short, not judgments and inferences because they admit of psychological analysis and explanation, any more than space is space because the perception of it can be worked out by genetic psychology. In other words, knowledge is first *knowledge* and only later a set of processes for psychological analysis. That is why, as it seems to me, all psychological logics, from Locke to our own day, have signally failed in dealing with the problem of knowledge. The attempt to construct knowledge out of mental states, the relations between ideas, and the relation of ideas to things, has been, as I read the history, decidedly without profit. Confusion and divergent opinion have resulted instead of agreement and confidence. On precisely the same psychological foundation, we have such divergent views of knowledge as idealism, phenomenalism and agnosticism with many other strange mixtures of logic, psychology and metaphysics. The lesson of these perplexing theories seems to be that logic, as logic, must be divorced from psychology.

It is also of importance to note, in this connection, that the determination of a process as mental and as thus falling within the domain of psychology strictly, has by no means been worked out to the general satisfaction of psychologists themselves. Recent literature abounds in elaborate discussion of the distinction between what is a mental fact and what not, with a pre-

vailing tendency to draw the remarkable conclusion that all facts are somehow mental or experienced facts. The situation would be worse for psychology than it is, if that vigorous science had not learned from other sciences the valuable knack of isolating concrete problems and attacking them directly, without the burden of previous logical or metaphysical speculation. Thus knowledge, which is the peculiar province of logic, is increased, while we wait for the acceptable definition of a mental fact. But definitions, be it remembered, are themselves logical matters. Indeed, some psychologists have gone so far as to claim that the distinction of a fact as mental is a purely 'logical' distinction. This is significant as indicating that the time has not yet come for the identification of logic and psychology.

In refreshingly sharp contrast to the vagueness and uncertainty which beset the definition of a mental fact, are the palpable concreteness and definiteness of knowledge itself. Every science, even history and philosophy, are instances of it. What constitutes a knowledge ought to be as definite and precise a question as could be asked. That logic has made no more progress than it has in the answer to it, appears to be due to the fact that it has not sufficiently grasped the significance of its own simplicity. Knowledge has been the important business of thinking man, and he ought to be able to tell what he does in order to know, as readily as he tells what he does in order to build a house. And that is why the Aristotelian logic has held its own so long. In that logic, 'the master of them that know' simply rehearsed the way he had systematized his own stores of knowledge. Naturally we, so far as we have followed his methods, have had practically nothing to add. In our efforts to improve on him, we have too often left the right way and followed the impossible method inaugurated by Locke. Had we examined with greater persistence our own methods of making science, we should have profited more. The introduction of psychology, instead of helping the situation, only confuses it.

Let it be granted, however, in spite of the vagueness of what is meant by a mental fact, that logical processes are also mental processes. This fact has, as I have already suggested, an important bearing on their genesis, and sets very definite limits to the freedom of thought in creating. It is not, however, as mental processes that they have the value of knowledge. A mental process which is knowledge purports to be connected with something other than itself, something which may not be a mental process at all. This connection should be investigated, but the investigation of it belongs, not to psychology, but to logic.

I am well aware that this conclusion runs counter to some metaphysical doctrines, and especially to idealism in all its forms, with the epistemologies based thereon. It is, of course, impossible here to defend my position by an elaborate analysis of these metaphysical systems. But I will say this. I am in entire agreement with idealism in its claim that questions of knowledge and of the nature of reality can not ultimately be separated, because we can know reality only *as* we know it. But the general question as to how we know reality can still be raised. By this I do not mean the question, how is it possible for us to have knowledge at all, or how it is possible for reality to be known at all, but how, as a matter of fact, we actually do know it? That we really do know it, I would most emphatically claim. Still further, I would claim that what we know about it is determined, not by the fact that we can know in general, but by the way reality, as distinct from our knowledge, has determined. These ways appear to me to be ascertainable, and form, thus, undoubtedly a section

of metaphysics. But the metaphysics will naturally be realistic rather than idealistic.

III.

Just as logical processes may be regarded as, at the same time, psychological processes, so they may be regarded, with equal right, as vital processes, coming thus under the categories of evolution. The tendency so to regard them is very marked at the present day, especially in France and in this country. In France, the movement has, perhaps received the clearer definition. In America the union of logic and biology is complicated—and at times even lost sight of—by emphasis on the idea of evolution generally. It is not my intention to trace the history of this movement, but I should like to call attention to its historic motive in order to get it in a clear light.

That the theory of evolution, even Darwinism itself, has radically transformed our historical, scientific and philosophical methods, is quite evident. Add to this the influence of the Hegelian philosophy, with its own doctrine of development, and one finds the causes of the rather striking unanimity which is discoverable in many ways between Hegelian idealists, on the one hand, and philosophers of evolution of Spencer's type, on the other. Although two men would, perhaps, not appear more radically different at first sight than Hegel and Spencer, I am inclined to believe that we shall come to recognize more and more in them an identity of philosophical conception. The pragmatism of the day is a striking confirmation of this opinion, for it is often the expression of Hegelian ideas in Darwinian and Spencerian terminology. The claims of idealism and of evolutionary science and philosophy have thus sought reconciliation. Logic has been, naturally, the last of the sciences to yield to evolutionary and genetic treatment. It could not escape long, especially when the idea of evolution had been so successful in its handling of ethics. If morality can be brought under the categories of evolution, why not thinking also? In answer to that question we have the theory that thinking is an adaptation, judgment is instrumental. But I would not leave the impression that this is true of pragmatism alone or that it has been developed only through pragmatic tendencies. It is naturally the result also of the extension of biological philosophy. In the biological conception of logic, we have, then, an interesting coincidence in the results of tendencies differing widely in their genesis.

It would be hazardous to deny, without any qualifications, the importance of genetic considerations. Indeed, the fact that evolution in the hands of a thinker like Huxley, for instance, should make consciousness and thinking apparently useless epiphenomena in a developing world, has seemed like a most contradictory evolutionary philosophy. It was difficult to make consciousness a real function in development so long as it was regarded as only cognitive in character. Evolutionary philosophy, coupled with physics, had built up a sort of closed system with which consciousness could not interfere, but which it could know, and know with all the assurance of a traditional logic. If, however, we were to be consistent evolutionists, we could not abide by such a remarkable result. The whole process of thinking must be brought within evolution, so that knowledge, even the knowledge of the evolutionary hypothesis itself, must appear as an instance of adaptation. In order to do this, however, consciousness must not be conceived as only cognitive. Judgment, the core of logical processes, must be regarded as an instrument and as a mode of adaptation.

The desire for completeness and consistency in an evolutionary philosophy is not the only thing which makes the denial

of genetic considerations hazardous. Strictly biological considerations furnish reasons of equal weight for caution. For instance, one will hardly deny that the whole sensory apparatus is a striking instance of adaptation. Our perceptions of the world would thus appear to be determined by this adaptation, to be instances of adjustment. They might conceivably have been different, and in the case of many other creatures, the perceptions of the world are undoubtedly different. All our logical processes, referring ultimately as they do to our perceptions, would thus appear finally to depend on the adaptation exhibited in the development of our sensory apparatus. So-called laws of thought would seem to be but abstract statements or formulations of the results of this adjustment. It would be absurd to suppose that a man thinks in a sense radically different from that in which he digests, or a flower blossoms, or that two and two are four in a sense radically different from that in which a flower has a given number of petals. Thinking, like digesting and blossoming, is an effect, a product, possibly a structure.

I am not at all interested in denying the force of these considerations. They have, to my mind, the greatest importance, and due weight has, as yet, not been given to them. To one at all committed to a unitary and evolutionary view of the world, it must indeed seem strange if thinking itself should not be the result of evolution, or that, in thinking, parts of the world had not become adjusted in a new way. But while I am ready to admit this, I am by no means ready to admit some of the conclusions for logic and metaphysics which are often drawn from the admission. Just because thought, as a product of evolution, is functional and judgment instrumental, it by no means follows that logic is but a branch of biology, or that knowledge of the world is but a temporary adjustment, which, as knowledge, might have been radically different. In these conclusions, often drawn with Protagorean assurance, two considerations of crucial importance seem to be overlooked, first, that adaptation is itself metaphysical in character, and secondly, that while knowledge may be functional and judgment instrumental, the character of the functioning has the character of knowledge, which sets it off sharply from all other functions.

It seems strange to me that the admission that knowledge is a matter of adaptation and thus a relative matter, should, in these days, be regarded as in any way destroying the claims of knowledge to metaphysical certainty. Yet, somehow, the opinion widely prevails that the doctrine of relativity necessarily involves the surrender of anything like absolute truth. 'All our knowledge is relative, and, therefore, only partial, incomplete, and but practically trustworthy,' is a statement repeatedly made. The fact that, if our development had been different, our knowledge would have been different, is taken to involve the conclusion, that our knowledge can not possibly disclose the real constitution of things, that it is essentially conditional, that it is only a mental device for getting results, that any other system of knowledge which would get results equally well would be equally true; in short, that there can be no such thing as metaphysical or epistemological truth. These conclusions do indeed seem strange, and especially strange on the basis of evolution. For while the evolutionary process might, conceivably, have been different, its results are, in any case, the results of the process. They are not arbitrary. We might have digested without stomachs, but the fact that we use stomachs in this important process ought not to free us from metaphysical respect for the organ. As M. Rey suggests, in the *Revue philosophique* for June, 1904, a creature without the sense

of smell would have no geometry, but that does not make geometry essentially hypothetical, a mere mental construction; for *we* have geometry because of the working out of nature's laws. Indeed, instead of issuing in a relativistic metaphysics of knowledge, the doctrine of relativity should issue in the recognition of the finality of knowledge in every case of ascertainably complete adaptation. In other words, adaptation is itself metaphysical in character. Adjustment is always adjustment between things, and yields only what it does yield. The things or elements get into the state which is their adjustment, and this adjustment purports to be their actual and unequivocal ordering in relation to one another. Different conditions might have produced a different ordering, but, again, this ordering would be equally actual and unequivocal, equally the *one* ordering to issue from them. To suppose or admit that the course of events might have been and might be different, is not at all to suppose or admit that it was or is different; it is, rather, to suppose and admit that we have real knowledge of what that course really was and is. This seems to be very obvious.

Yet the evolutionist often thinks that he is not a metaphysician, even when he brings all his conceptions systematically under the conception of evolution. This must be due to some temporary lack of clearness. If evolution is not a metaphysical doctrine when extended to apply to all science, all morality, all logic, in short, all things, then it is quite meaningless for evolutionists to pronounce a metaphysical sentence on logical processes. But if evolution is a metaphysics, then its sentence is metaphysical, and in every case of adjustment or adaptation we have a revelation of the nature of reality in a definite and unequivocal form. This conclusion applies to logical processes as well as to others. The recognition that they are vital processes can, therefore, have little significance for these processes in their distinctive character as logical. They are like all other vital processes in that they are vital and subject to evolution. They are unlike all others in that thought is unlike digestion or breathing. To regard logical processes as vital processes does not in any way, therefore, invalidate them as logical processes or make it superfluous to consider their claim to give us real knowledge of a real world. Indeed, it makes such a consideration more necessary and important.

A second consideration overlooked by the Protagorean tendencies of the day is, that judgment, even if it is instrumental, purports to give us knowledge, that is, it claims to reveal what is independent of the judging process. Perhaps I ought not to say that this consideration is overlooked, but rather that it is denied significance. It is even denied to be essential to judgment. It is claimed that, instead of revealing anything independent of the judging process, judgment is just the adjustment and no more. It is a reorganization of experience, an attempt at control. All this looks to me like a misstatement of the facts. Judgment *claims* to be no such thing. It does not function as such a thing. When I make any judgment, even the simplest, I may make it as the result of tension, because of a demand for reorganization, in order to secure control of experience; but the judgment *means* for me something quite different. It means decidedly and unequivocally that in reality, apart from the judging process, things exist and operate just as the judgment declares. If it is claimed that this meaning is illusory, I eagerly desire to know on what solid ground its illusoriness can be established. When the conclusion was reached, that gravitation varies directly as the mass and inversely as the square of the distance, it was doubtless reached in an evolutionary and prag-

matic way; but it claimed to disclose a fact which prevailed before the conclusion was reached, and in spite of the conclusion. Knowledge has been born of the travail of living, but it has been born as knowledge.

When the knowledge character of judgment is insisted on, it seems almost incredible that anyone would think of denying or overlooking it. Indeed, current discussions are far from clear on the subject. Pragmatists are constantly denying that they hold the conclusions that their critics almost unanimously draw. There is, therefore, a good deal of confusion of thought yet to be dispelled. Yet there seems to be current a pronounced determination to banish the epistemological problem from logic. This is, to my mind, suspicious, even when epistemology is defined in a way which most epistemologists would not approve. It is suspicious just because we must always ask eventually that most epistemological and metaphysical question: 'Is knowledge true?' To answer, it is true when it functions in a way to satisfy the needs which generated its activity, is, no doubt, correct, but it is by no means adequate. The same answer can be made to the inquiry after the efficiency of any vital process whatever, and is, therefore, not distinctive. We have still to inquire into the specific character of the needs which originate judgments and of the consequent satisfaction. Just here is where the uniqueness of the logical problem is disclosed. With conscious beings, the success of the things they do has become increasingly dependent on their ability to discover what takes place in independence of the knowing process. That is the need which generates judgment. The satisfaction is, of course, the attainment of the discovery. Now to make the judgment itself and not the consequent action the instrumental factor, seems to me to misstate the facts of the case. Nothing is clearer than that there is no necessity for knowledge to issue in adjustment. And it is clear to me, that increased control of experience, while resulting from knowledge, does not give to it its character. Omniscience could idly view the transformations of reality and yet remain omniscient. Knowledge works, but it is not, therefore, knowledge.

These considerations have peculiar force when applied to that branch of knowledge which is knowledge itself. Is the biological account of knowledge correct? That question we must evidently ask, especially when we are urged to accept the account. Can we, to put the question in its most general form, accept as an adequate account of the logical process a theory which is bound up with some other specific department of human knowledge? It seems to me that we cannot. Here we must be epistemologists and metaphysicians, or give up the problem entirely. This by no means involves the attempt to conceive pure thought set over against pure reality—the kind of epistemology and metaphysics justly ridiculed by the pragmatist—for knowledge, as already stated, is given to us in concrete instances. How knowledge in general is possible, is, therefore, as useless and meaningless a question as how reality in general is possible. The knowledge is given as a fact of life, and what we have to determine is not its non-logical antecedents or its practical consequences, but its constitution as knowledge and its validity. It may be admitted that the question of validity is settled pragmatically. No knowledge is true unless it yields results which can be verified, unless it *can* issue in increased control of experience. But I insist again, that that fact is not sufficient for an account of what knowledge claims to be. It claims to issue in control because it is true in independence of the control. And it is just this assurance that is needed to distinguish knowledge from what is not knowledge. It is the ne-

cessity of exhibiting this assurance which makes it impossible to subordinate logical problems, and forces us at last to questions of epistemology and metaphysics.

As I am interested here primarily in determining the field of logic, it is somewhat outside my province to consider the details of logical theory. Yet the point just raised is of so much importance in connection with the main question, that I venture the following general considerations. This is, perhaps, the more necessary because the pragmatic doctrine finds in the concession made regarding the test of validity one of its strongest defences.

Of course a judgment is not true simply because it is a judgment. It may be false. The only way to settle its validity is to discover whether experience actually provides what the judgment promises, that is, whether the conclusions drawn from it really enable us to control experience. No mere speculation will yield the desired result, no matter with how much formal validity the conclusions may be drawn. That merely formal validity is not the essential thing, I have pointed out in discussing the relation of logic to mathematics. The test of truth is pragmatic. It is apparent, therefore, that the formal validity does not determine the actual validity. What is this but the statement that the process of judgment is not itself the determining factor in its real validity? It is, in short, only valid judgments that can really give us control of experience. The implications taken up in the judgment must, therefore, be real implications which, as such, have nothing to do with the judging process, and which, most certainly, are not brought about by it. And what is this but the claim that judgment as such is never instrumental? In other words, a judgment which affected its own content would only by the merest accident function as valid knowledge. We have valid knowledge, then, only when the implications of the judgment are found to be independent of the judging process. We have knowledge only at the risk of error. The pragmatic test of validity, instead of proving the instrumental character of judgment, would thus appear to prove just the reverse.

Valid knowledge has, therefore, for its content a system of real, not judged or hypothetical implications. The central problem of logic which results from this fact is not how a knowledge of real implications is then possible, but what are the ascertainable types of real implications. But, it may be urged, we need some criterion to determine what a real implication is. I venture to reply that we need none, if by such is meant anything else than the facts with which we are dealing. I need no other criterion than the circle to determine whether its diameters are really equal. And, in general, I need no other criterion than the facts dealt with to determine whether they really imply what I judge them to imply. Logic appears to me to be really as simple as this. Yet there can be profound problems involved in the working out of this simple procedure. There is the problem already stated of the most general types of real implication, or, in other words, the time honored doctrine of categories. Whether there are categories or basal types of existence, seems to me to be ascertainable. When ascertained, it is also possible to discover the types of inference or implication which they afford. This is by no means the whole of logic, but it appears to me to be its central problem.

These considerations will, I hope, throw light on the statement, that while knowledge works, it is not therefore knowledge. It works because its content existed before its discovery by the knowledge process, and because its content was not effected or brought about by that process. Judgment was the instrument of its discovery, not the instru-

ment which fashioned it. While, therefore, willing to admit that logical processes are vital processes, I am not willing to admit that the problem of logic is radically changed thereby in its formulation or solution, for the vital processes in question have the unique character of knowledge, the content of which is what it claims to be, a system of real implications which existed prior to its discovery.

In the psychological and biological tendencies in logic, there is, however, I think, a distinct gain for logical theory. The insistence that logical processes are both mental and vital has done much to take them out of the transcendental aloofness from reality in which they have often been placed, especially since Kant. So long as thought and object were so separated that they could never be brought together, and so long as logical processes were conceived wholly in terms of ideas set over against objects, there was no hope of escape from the realm of pure hypothesis and conjecture. Locke's axiom that 'the mind, in all its thoughts and reasonings, hath no other immediate object but its own ideas,' an axiom which Kant did so much to sanctify, and which has been the basal principle of the greater part of modern logic and metaphysics, is most certainly subversive of logical theory. The transition from ideas to anything else is rendered impossible by it. Now it is just this axiom which the biological tendencies in logic have done so much to destroy. They have insisted, with the greatest right, that logical processes are not set over against their content as idea against object, as appearance against reality, but are processes of reality itself. Just as reality can and does function in a physical or a physiological way, so also it functions in a logical way. The state we call knowledge becomes, thus, as much a part of the system of things as the state we call chemical combination. The problem how thought can know anything becomes, therefore, as irrelevant as the problem how elements can combine at all. The recognition of this is a great gain, and the promise of it most fruitful for both logic and metaphysics.

But, as I have tried to point out, all this surrendering of pure thought as opposed to pure reality, does not at all necessitate our regarding judgment as a process which makes reality different from what it was before. Of course there is one difference, namely, the logical one; for reality prior to logical processes is unknown. As a result of these processes it becomes known. These processes are, therefore, responsible for a known as distinct from an unknown reality. But what is the transformation which reality undergoes in becoming known? When it becomes known that water seeks its own level, what change has taken place in the water? It would appear that we must answer, none. The water which seeks its own level has not been transformed into ideas or even into a human experience. It appears to remain, as water, precisely what it was before. The transformation which takes place, takes place in the one who knows, a transformation from ignorance to knowledge. Psychology and biology can afford us the natural history of this transformation, but they can not inform us in the least as to why it should have its specific character. That is given and not deduced. The attempts to deduce it have, without exception, been futile. That is why we are forced to take it as ultimate in the same way we take as ultimate the specific character of any definite transformation. To my mind, there is needed a fuller and more cordial recognition of this fact. The conditions under which we, as individuals, know, are certainly discoverable, just as much as the conditions under which we breathe or digest. And what happens to

things when we know them, is also as discoverable as what happens to them when we breathe them or digest them.

But here the idealist may interpose that we can never know what happens to things when we know them, because we can never know them before they become known. I suppose I ought to wrestle with this objection. It is an obvious one, but, to my mind, it is without force. The objection, if pursued, can carry us only in a circle. The problem of knowledge is still on our hands, and every logician of whatever school, the offerer of this objection also, has, nevertheless, attempted to show what the transformation is that thought works, for all admit that it works some. Are we, therefore, engaged in a hopeless task? Or have we failed to grasp the significance of our problem? I think the latter. We fail to recognize that, in one way or other, we do solve the problem, and that our attempts to solve it show quite clearly that the objection under consideration is without force. Take for instance, any concrete case of knowledge, the water seeking its own level, again. Follow the process of knowledge to the fullest extent, we never find a single problem which is not solvable by reference to the concrete things with which we are dealing, nor a single solution which is not forced upon us by these things rather than by the fact that we deal with them. The transformation wrought is thus discovered, in the progress of knowledge itself, to be wrought solely in the inquiring individual, and wrought by repeated contact with the things with which he deals. In other words, all knowledge discloses the fact that its content is not created by itself, but by the things with which it is concerned.

It is quite possible, therefore, that knowledge should be what we call transcendent and yet not involve us in a transcendental logic. That we should be able to know without altering the things we know, is no more and no less remarkable and mysterious than that we should be able to digest by altering the things we digest. In other words, the fact that digestion alters the things is no reason that knowledge should alter them, even if we admit that logical processes are vital and subject to evolution. Indeed, if evolution teaches us anything on this point, it is that knowledge processes are real just as they exist, as real as growth and digestion, and must have their character described in accordance with what they are. The recognition that knowledge can be transcendent and yet its processes vital, seems to throw light on the difficulty evolution has encountered in accounting for consciousness and knowledge. All the reactions of the individual seem to be expressible in terms of chemistry and physics without calling in consciousness as an operating factor. What is this but the recognition of its transcendence, especially when the conditions of conscious activity are quite likely expressible in chemical and physical terms? While, therefore, biological considerations result in the great gain of giving concrete reality to the processes of knowledge, the gain is lost, if knowledge itself is denied the transcendence which it so evidently discloses.

IV.

The argument advanced in this discussion has had the aim of emphasizing the fact, that in knowledge we have actually given, as content, reality as it is in independence of the act of knowing, that the real world is self-existent, independent of the judgments we make about it. This fact has been emphasized in order to confine the field of logic to the field of knowledge as thus understood. In the course of the argument, I have occasionally indicated what some of the resulting problems of

logic are. These I wish now to state in a somewhat more systematic way.

The basal problem of logic becomes, undoubtedly, the metaphysics of knowledge, the determination of the nature of knowledge and its relation to reality. It is quite evident that this is just the problem which the current tendencies criticized have sought, not to solve, but to avoid or set aside. Their motives for so doing have been mainly the difficulties which have arisen from the Kantian philosophy in its development into transcendentalism, and the desire to extend the category of evolution to embrace the whole of reality, knowledge included. I confess to feeling the force of these motives as strongly as any advocate of the criticized opinions. But I do not see my way clear to satisfying them by denying or explaining away the evident character of knowledge itself. It appears far better to admit that a metaphysics of knowledge is as yet hopeless, rather than so to transform knowledge as to get rid of the problem; for we must ultimately ask after the truth of the transformation. But I am far from believing that a metaphysics of knowledge is hopeless. The biological tendencies themselves seem to furnish us with much material for at least the beginnings of one. Reality known is to be set over against reality unknown or independent of knowledge, not as image to original, idea to thing, phenomena to noumena, appearance to reality; but reality as known is a new stage in the development of reality itself. It is not an external mind which knows reality by means of its own ideas, but reality itself becomes known through its own expanding and readjusting processes. So far I am in entire agreement with the tendencies I have criticised. But what change is effected by this expansion and readjustment? I can find no other answer than this simple one: the change to knowledge. And by this I mean to assert unequivocally that the addition of knowledge to a reality hitherto without it, is simply an addition to it and not a transformation of it. Such a view may appear to make knowledge a wholly useless addition, but I see no inherent necessity in such a conclusion. Nor do I see any inherent necessity of supposing that knowledge must be a useful addition. Yet I would not be so foolish as to deny the usefulness of knowledge. We have, of course, the most palpable evidences of its use. As we examine them, I think we find, without exception, that knowledge is useful just in proportion as we find that reality is not transformed by being known. If it really were transformed in that process, could anything else than confusion result from the multitude of knowing individuals?

To me, therefore, the metaphysics of the situation resolves itself into the realistic position, that a developing reality develops, under ascertainable conditions, into a known reality without undergoing any other transformation, and that this new stage marks an advance in the efficiency of reality in its adaptations. My confidence steadily grows that this whole process can be scientifically worked out. It is impossible here to justify my confidence in detail, and I must leave the matter with the following suggestion. The point from which knowledge starts and to which it ultimately returns, is always some portion of reality where there is consciousness, the things, namely, which, we are wont to say, are in consciousness. These things are not ideas representing other things outside of consciousness, but real things, which, by being in consciousness, have the capacity of representing *each other*, of standing for or implying each other. Knowledge is not the creation of these implications, but their successful systematization. It will be found, I think, that this general statement

is true of every concrete case of knowledge which we possess. Its detailed working out would be a metaphysics of knowledge, an epistemology.

Since knowledge is the successful systematization of the implications which are disclosed in things by virtue of consciousness, a second logical problem of fundamental importance is the determination of the most general types of implication with the categories which underlie them. The execution of this problem would naturally involve, as subsidiary, the greater part of formal and symbolic logic. Indeed, vital doctrines of the syllogism, of definition, of formal inference, of the calculus of classes and propositions, of the logic of relations, appear to be bound up ultimately with a doctrine of categories; for it is only a recognition of basal types of existence with their implications, that can save these doctrines from mere formalism. These types of existence or categories are not to be regarded as free creations or as the contributions of the mind to experience. There is no 'deduction' of them possible. They must be discovered in the actual progress of knowledge itself, and I see no reason to suppose that their number is necessarily fixed, or that we should necessarily be in possession of all of them. It is requisite, however, that in every case categories should be incapable of reduction to each other.

A doctrine of categories seems to me to be of the greatest importance in the systematization of knowledge, for no problem of relation is even statable correctly, before the type of existence to which its terms belong has been first determined. I submit one illustration to reinforce this general statement, namely, the relation of mind to body. If mind and body belong to the same type of existence, we have one set of problems on our hands, but if they do not, we have an entirely different set. Yet volumes of discussion written on this subject have abounded in confusion, simply because they have regarded mind and body as belonging to radically different types of existence and yet related in terms of the type to which one of them belongs. The doctrine of 'parallelism' is, perhaps, the epitome of this confusion.

The doctrine of categories will involve not only the greater part of formal and symbolic logic, but will undoubtedly carry the logician into the doctrine of method. Here it is to be hoped that recent tendencies will result in effectively breaking down the artificial distinctions which have prevailed between deduction and induction. Differences in method do not result from differences in points of departure, or between the universal and the particular, but from the categories, again, which give the method direction and aim, and result in different types of synthesis. In this direction, the logician may hope for an approximately correct classification of the various departments of knowledge. Such a classification is, perhaps, the ideal of logical theory.

Frederick J. E. Woodbridge.
Columbia University.

CLARENCE L. HERRICK.

The death of Professor Clarence L. Herrick September 15 in New Mexico was noted in Science for September 23. In him neurology and geology alike have lost a brilliant investigator and a teacher of rare power.

His scientific work began in the high school. During his college course at the University of Minnesota, where he graduated in 1880, he was employed on the Natural History Survey of the state and for five years following he was actively connected with this work, completing a large quarto on the Mammals of Minnesota in 1885. From 1884 to 1889 and again from

1892 to 1894 he held a professorship at Denison University. Here he continued his zoological studies, but in connection with his class work became interested in the Waverly limestones and shales of Ohio. He devoted himself to the study of these for several years with characteristic intensity, publishing most of his results and those of his students in the *Bulletin of the Scientific Laboratories of Denison University,* which he founded in 1885. From the first his teaching was extraordinarily successful, particularly in kindling enthusiasm and love of research. This was due partly to his attractive personality, partly to his fearless originality, but chiefly to his philosophic insight and his ability to open up his deepest thinking even to elementary pupils. And so a very large proportion of his students have themselves achieved success as original workers in science.

During his last ten years, spent in New Mexico on account of the breakdown in health which forced him to leave Ohio, he resumed his geological studies, publishing several important articles on the geology of that territory.

From 1889 to 1891 he was professor of zoology in the University of Cincinnati. Here his geological labors were interrupted and he entered with great energy into a series of neurological investigations which he had long before planned to undertake. He founded the *Journal of Comparative Neurology and Psychology,* which (now under the editorship of his brother, C. Judson Herrick) has made a permanent and important place for itself. Beginning his neurological work upon the brain of rodents, he accumulated a large mass of data which he found almost incapable of correlation. Believing that the key could be found only in lower primitive types he began to examine a large number of such in a very thorough manner. His results were published rapidly and with little attempt at correlation. These papers were illustrated by large numbers of beautiful plates which his rapid and skillful use of the pencil made possible. His plan was to secure a large amount of accurate data while his eyesight was still perfect, and later review the whole field of vertebrate neurology, using his own observations as a nucleus around which to build a unified system by further research at critical points. In 1892, after some months of study abroad, he returned to Denison and continued his neurological work with great energy, until in December 1893 failing health compelled him to go to New Mexico. He soon recovered sufficiently to resume work, but local conditions were such that his attention was again directed mainly to geological problems. The work of correlating his neurological studies was left somewhat incomplete, but it is probable that this may be accomplished through the labors of his brain-children.

For four years he did a useful work as president of the University of New Mexico, here as everywhere stimulating young men to undertake research by the influence of his own example. In these later years we see the successful struggle of a noble soul dominated by a great purpose over the discouragements of physical weakness and suffering.

A. D. COLE.

SCIENTIFIC BOOKS.

The Harriman Alaska Expedition. Vols. VIII. and IX., Insects. New York, Doubleday, Page & Co. 1904.

Volumes VIII. and IX. of the Harriman Alaska Expedition, published in cooperation with the Washington Academy of Sciences and dealing with the subject of insects, have just been issued, and represent the most notable contribution to the literature of entomology of the year, and will rank among the most important of the American contributions to this subject. The material discussed in these two volumes was substantially all of it col-

lected by the Entomologist of the Expedition, Professor Trevor Kincaid, of the chair of biology in the University of the State of Washington. Professor Kincaid is known as an expert student and collector of insects, and his work on this expedition demonstrates his ability in this field. More than 8,000 insects were collected, representing a thousand and one species, 344 of which are new to science, and including at least half a dozen new genera. Upon the return of the expedition the collections were carefully labeled and sorted into groups by Professor Kincaid and transmitted to Dr. L. O. Howard, chief of the Bureau of Entomology, U. S. Department of Agriculture, and honorary curator of insects in the National Museum, for distribution to specialists for study and report. The results are given in eighteen important papers. In most instances these contributions deal only with the material collected in Alaska by Professor Kincaid. In the Myriapoda, however, all the available data relating to northwestern North America is brought together, this paper, therefore, representing the present knowledge in this group for the region designated. In some other cases, as in the Hymenoptera, the previously known records from Alaska are added. A number of the papers were published in the 'Proceedings of the Washington Academy of Sciences,' but are here reprinted from the same electrotype plates, an explanatory page accompanying each of these last mentioned papers.

Insecta.	Described Species.	New Species.	Total.
Apterygota	8	6	14
Neuropteroids	25	9	34
Odonata	8	0	8
Orthoptera	1	0	1
Hemiptera { Heteroptera	18	0	18
Hemiptera { Homoptera	14	10	24
Coleoptera	154	1	155
Diptera	213	63	276
Lepidoptera	66	9	75
Hymenoptera	98	237	335
Myriapoda	6	3	9
Arachnida	46	6	52
Total	657	344	1,001

The results of this expedition are indicated in the table given above, in which are listed the previously described, the new species and the total number of species collected, arranged by orders.

The itinerary of the trip given in the introduction is very interesting reading. It describes the localities visited from day to day, indicating the predominating vegetation and the more important insects found, and is a most valuable supplement to the more technical succeeding papers.

The original idea was to publish a single volume of insects, but when the papers were assembled it was found necessary to publish the insect portion in two volumes. The largest number of species were found in the orders Diptera and Hymenoptera, and the two more important papers are those dealing especially with these two groups, namely, the paper on Diptera, by Mr. Coquillett, and on Hymenoptera, by Mr. Ashmead, the former considering 276 species, and the latter 335 species. Mr. Ashmead has included in his paper all the known Hymenoptera previously reported from Alaska, and also certain unstudied Alaskan material represented in the National Museum collections. The extent of the additions in this order will be understood from the statement that previous to the publication of this paper but 30 species of Hymenoptera were known from Alaska. Two hundred and one of the 335 species listed by Mr. Ashmead are described as new.

A complete list of the subjects and authors follows: 'Introduction,' by Trevor Kincaid; 'Arachnida,' by Nathan Banks; 'Myriapoda,' by O. F. Cook; 'Apterygota,' by Justus Watson Folsom; 'Orthoptera,' by A. N. Caudell; 'Aphididæ,' by Theo. Pergande; 'Homoptera,' by William H. Ashmead; 'Heteroptera,' by O. Heidemann; 'Odonata,' by Rolla P. Currie; 'Neuropteroid Insects,' by Nathan Banks; 'Coleoptera,' by E. A. Schwarz; 'Metamorphoses of Alaska Coleoptera,' by Trevor Kincaid; 'Lepidoptera,' by Harrison G. Dyar; 'Diptera,' by D. W. Coquillett; 'Tenthredinoidea,' by Trevor Kincaid; 'Sphegoidea and Vespoidea,' by Trevor Kincaid; 'Formicidæ,' by Theo. Pergande; 'Hymenoptera,' by William H. Ashmead.

Each volume is separately indexed, and

many of the papers are illustrated with lithographic plates. All have appropriate head figures, and there are occasional text figures. Part I., Vol. VIII., has a handsome colored plate in which representative insects of different orders are figured, and Part II., Vol. IX., has, as a frontispiece, a heliotype print of Popof Island. The style of printing, binding and illustration is a testimony to the experience and interest of the editor, Dr. C. Hart Merriam, and of the publishers, Doubleday, Page & Co.

C. L. MARLATT.
U. S. DEPARTMENT OF AGRICULTURE.

Lehrbuch der vergleichenden Anatomie. By B. HALLER. Zweite Lieferung. Jena, Gustav Fischer. 1904.

A little more than a year ago a review of the first part of Haller's 'Lehrbuch der vergleichenden Anatomie' was published in this journal (SCIENCE, September 13, 1903) and we have now before us the second part, dealing with the chordata, a volume of some 579 large octavo pages and containing 465 figures.

As is but natural, the standpoint of the author is that of the Heidelberg school and his treatment of the subject is essentially the same as that presented by Gegenbaur in his 'Vergleichenden Anatomie der Wirbelthiere,' so much so, indeed, that the present volume may be regarded as very largely an abridgment of the more extensive work. As such it will undoubtedly prove useful, especially as the author's style is generally clear, the reading pleasant and the figures numerous, excellently reproduced and pertinent.

To speak of the volume as an efficient abridgment of Gegenbaur's 'Vergleichenden Anatomie' is no inconsiderable praise, but it also implies that a perusal of the book gives one the impression that little has been added to our knowledge of the comparative anatomy of the vertebrates since Gegenbaur's volumes appeared. It is unfortunate, also, that the author has chosen to express dogmatically certain hypotheses which, to say the least, are still *sub judice*. Thus, for example, the dogmatic reference (p. 465) of all cases of polymastia in the human species of atavism is certainly ill-advised, as is also the apparently unconditional acceptance of the explanation of the double articulations of the ribs as a relic of a primary double-ribbed condition, a theory which lacks at present any embryological confirmation. So too the emphasized homology (p. 512) of the præchordal portion of the head with the præoral lobe of annelids is decidedly open to adverse criticism and places the author, it may be noted, on a very different standpoint from that adopted by Gegenbaur, for whom the præchordal portion of the cranium was a secondary structure developed from the primitive chordal cranium in adaptation to the development of the brain and sense-organs.

An efficient treatment of the vertebrate nervous system is to be expected from Professor Haller, and in some respects, notably in that some attention is paid to the tracts of the central system, the section shows an improvement over what is usually furnished by text-books of the grade of the present one. A little too much is attempted, unfortunately, within the limits set for the section, the result being an occasional obscurity, but a far more serious defect is the failure to discuss the cranial nerves on the basis of their components. Nothing that has been added to our knowledge of the nervous system within recent years equals in morphological significance the recognition of the cranial nerve components, and any discussion of the cranial nerves on the basis of the old two-root hypothesis must be futile so far as a proper understanding of their general morphology is concerned. And yet not a word is to be found in the volume under review concerning nerve components, but merely one of the variants of the oft-repeated two-root theory, which for many years has but served to retard progress towards the solution of the problem of vertebrate cephalogenesis.

More excusable is the insufficient treatment afforded the sympathetic system, for in it the author is but following the examples of his predecessors, no adequate account of that physiologically important system being as yet incorporated in any text-book of vertebrate anatomy. But scant consideration is given

to the thyreoid and thymus glands, and the difficult morphology of the cœlom and peritoneum is dismissed with little more than a page and a half of actual reading matter.

In the review of the first part of the 'Lehrbuch' attention was called to the exceptionally numerous and gross typographical errors it contained. Two pages of errata contained in the first part form an introduction to the second part, and aroused hopes that due care had been taken that the latter should be tolerably free from errors of this sort. The hopes, however, were vain. Although the errors are less numerous and striking than in the earlier part yet they are sufficiently abundant to cause surprise. They are, as before, most frequent in connection with the proper names, but one must needs rub one's eyes and look again when one reads of the lamina scribosa of the ethmoid or of the hetopapancreas. J. P. McM.

SOCIETIES AND ACADEMIES.

THE SAN FRANCISCO SECTION OF THE AMERICAN MATHEMATICAL SOCIETY.

THE sixth regular meeting of the San Francisco Section of the American Mathematical Society was held at the Universty of California on October 1, 1904. Fifteen members of the society were present. A number of other teachers of mathematics living in or near San Francisco attended both of the sessions. The following officers were elected for the ensuing year:

Chairman—Professor Haskell.

Secretary—Professor Miller.

Program Committee—Professors Haskell, Lehmer and Miller.

Major P. A. MacMahon member of the council of the London Mathematical Society, presided during the morning session, and Professor Irving Stringham during the afternoon session. The following papers were read:

PROFESSOR E. J. WILCZYNSKI: 'General projective theory of space curves.'

PROFESSOR L. M. HOSKINS: 'Stresses in an elastic sphere due to a superficial layer of heavy matter of uniform thickness bounded by a circle.'

DR. T. M. PUTNAM: 'Concerning the factors of $(p^2-1)^2$ that are of the form $px+1$, and allied problems.'

MAJOR P. A. MACMAHON: 'Groups of linear differential operators.'

PROFESSOR H. F. BLICHFELDT: 'On primitive continuous groups.'

PROFESSOR D. N. LEHMER: 'Figures invariant in space of three dimensions under the most general projective transformation.'

PROFESSOR G. A. MILLER: 'Determination of all the groups of order 2^m which contain an odd number of cyclic subgroups of composite order.'

The program provided also for a 'Conference on recent investigations in the foundations of geometry.' This conference was opened by Professor Stringham, who was followed by Dr. J. H. McDonald. A number of the high school teachers took part in the discussion, which had reference mainly to the influence of the recent investigations on the teaching of elementary mathematics. The next meeting will be held at Stanford University, on February 25, 1905.

G. A. MILLER,
Secretary of the Section.

NEW YORK ACADEMY OF SCIENCES.
SECTION OF GEOLOGY AND MINERALOGY.

THE section met at the American Museum of Natural History on October 17, 1904, at 8:15 P.M., with Professor James F. Kemp in the chair.

The special business of the evening was the nomination by the section of officers to serve for the calendar year 1905. The following nominations were made: For chairman and vice-president of the academy, E. O. Hovey, of the American Museum of Natural History; for secretary, A. W. Grabau, of Columbia University.

The program of the evening consisted of a lecture by E. O. Hovey, on 'St. Vincent, British West Indies: The Eruptions of 1902 and their Immediate Results.'

The author gave a summary account of the results obtained on two expeditions undertaken by him for the American Museum of Natural History in 1902 and 1903, for the study of the volcanic eruptions of the Soufrière which began in May, 1902. Particular attention was devoted to the heavy coating of

volcanic ash deposited upon the northern portion of the island of St. Vincent and the ash-filling of the gorges of the Wallibou and Rabaka Dry Rivers, the devastation wrought in the forests and on the plantations within a radius of about five miles from the crater and the phenomena of primary eruptions observed in the crater and of secondary eruptions observed in the Wallibou and Rabaka ash-beds. The nature of the exploding eruption cloud was discussed and it was shown how the heavily dust-laden steam cloud kept close to the surface of the ground under the influence of gravity while its initial velocity was furnished by the horizontal component of the explosion.

About eighty lantern slides were used in illustrating the speaker's remarks.

EDMUND OTIS HOVEY,
Secretary.

THE ELISHA MITCHELL SCIENTIFIC SOCIETY OF THE UNIVERSITY OF NORTH CAROLINA.

THE 155th meeting was held in the Chemical Lecture Room, Tuesday, October 11, 7:30 P.M. The following papers were given: 'The Construction of a Double Six,' by Professor Archibald Henderson; 'The Geological History of Currituck Banks,' by Professor Collier Cobb.

ALVIN S. WHEELER,
Recording Secretary.

DISCUSSION AND CORRESPONDENCE.

SOIL MANAGEMENT.*

"The three papers here printed have been refused departmental publication by the Chief of the Bureau of Soils."

In glancing at this note on the title page of this pamphlet of 168 pages, the reader is naturally struck with the query, why the U. S. Department of Agriculture should decline to publish the results of the work of such a man as King, working under its auspices. Has the salt indeed lost its savor? Both American and European scientists have been accustomed for many years to regard with confidence and respect the work and publications of the man upon whom, by common consent, the mantle of Wollny has fallen since the premature death of the soil physicist of Germany. It is certainly worth the while of every worker in agricultural science to see and judge for himself whether a star has been eclipsed or blotted out from the scientific firmament, and if so, from what cause.

We are, at the outset, somewhat reassured as to the totality of the conjectured eclipse, by finding that the three rejected bulletins are but a portion of a series of six forming the report of King, as head of the Division of Soil Management, for the years 1902 and 1903. Since three out of the six have been accepted by the department for publication, it is evident that King's right hand has not wholly lost its cunning during these two years. What, then, is the matter with Bulletins D, E and F, here presented to us by the author at his personal expense and risk, and as he expressly states, in their original form?

As it happens, the rest of the series, bulletins B, C and G, have not yet reached publication by the bureau of soils. We must, therefore, rely upon the intrinsic evidence contained in the three now before us, to settle the reason for their rejection.

In his preface the author reticently says that the 'adequate discussion was withheld in order to avoid, as far as possible, antagonizing the published views of the Bureau' (of Soils); and hence the three papers are published without general comments. It is to the conclusions deducible from the facts given, then, that we must look for the substance of these papers, and for the possible cause of their falling under condemnation.

Bulletin E, the first in the pamphlet and the most important of the three, treats of the results obtained in the fertilization with stable manure, in different multiple proportions, of eight different types of soils. The experiments were conducted on eight two-acre plots, located respectively near Goldsboro, N. C., Upper Marlboro, Md., Lancaster, Pa., and Janesville, Wis., and representing two groups

* 'Investigations in Soil Management,' being three of six papers on the influence of soil management upon the water-soluble salts in soils, and the yield crops, by F. H. King, Madison, Wisconsin. Published by the author, with permission of the Secretary of Agriculture.

of four each, 'strongly contrasted in their native productive capacities, in order that strongly marked differences might be dealt with.' The dressings of barnyard manure used were at the rate of five, ten and fifteen tons per acre. The crops grown were potatoes and corn, with a series of unmanured check-plots between, in each case.

The crops from each series of plots were weighed, mostly both in the green and in the dry condition; and concurrently, the kinds and amounts of soluble salts extractable by water from the soils of each of the plots before and at different intervals after the application of the manure, were determined according to the delicate methods used in the investigations of aqueous soil extracts.* Moreover, the amounts of the several substances contained in the soil extracts, present in the sap of the plants themselves, were likewise determined, in order to ascertain the relations between the soil solutions and the substances taken up by the crops.

It is not easy for the outsider to detect anything reprehensible in this well-considered plan of operations. It seems to be admirably conceived for the determination of the relation of the soil solutions to plant nutrition and crop production under normal, practical conditions. The details given regarding the actual carrying out of the experiments are equally unexceptionable, except as concerns some points in respect to which, apparently, there was interference of some sort with the plan; *e. g.*, in the matter of making chemical analyses of the stable manure used at the several localities. But however regrettable, this and some other omissions, apparently imposed by superior authority, do not vitiate, to any material extent, the conclusions arrived at by King.

The plan and methods of experimentation being thus unexceptionable so far as any one examining the record given can judge, the only question remaining is whether the conclusions deduced from the experimental results are justified, and whether these are in conflict with practical or scientific experience, or with common sense. Of these conclusions it will be best to give the chief ones in the words of the author.

* Bulletin No. 22, Bureau of Soils.

After giving, on page 5, a table showing the percentage relations of crop yield under different fertilizations, he says: 'It will be seen that in the case of the poorer soils there is a percentage difference of 46 between the yields of the fifteen-ton subplots and those to which nothing has been added; but a difference of only eighteen on the stronger soils.' Recalculating these results on the next page so as to show their relations more clearly, he adds: 'These results show that both relatively and absolutely, adding fertilizers to the poorer soils has had a greater effect than the same treatment with stronger soils.' Farther on, after giving a table of the several yields of water-free shelled corn, he says: "It is here seen that on the four poorer soils, there is a systematic difference in the yield of water-free shelled corn, closely related to the fertilizers applied to the soil. The group of four stronger soils do not show, throughout, this systematic relation." Photographic views of the corn on the growing plots show these differences clearly in the growth of the plants.

The only criticism that could be, perhaps, made of the work leading to these conclusions from an outside point of view, is that they are so clearly and thoroughly in accord with all former experience, both practical and experimental, that they are largely foreseen.

Then follows the record and discussion of corresponding experiments with potatoes, which yield practically the same results and conclusions.

Then are given the results of analyses of leachings of the same soils upon which these crops had been grown. The results are presented in a table, from which "it is very clear that the effect of different amounts of stable manure applied to these soils * * * has been such upon the recovery of the water-soluble salts as to enable the same treatment to remove different amounts from different fertilizations. * * * There is a clear quantitative relation, too, between the yields and the salts recovered, these (the former) increasing where the essential ingredients of plant food are higher."

King also details the experiments made with small (four-pound) samples of soils mixed with much larger amounts of the same manure, the leachings of which after 65 days, gave, in general, results corresponding to those obtained from the field tests; and he discusses in detail the apparent effects upon the solubilities of the several ingredients of plant food, and the influence upon the formation and reduction of nitrates; showing that there is no direct ratio between the amount of manure added and the nitrates found in the different soils. He determines and discusses, likewise, the relation of the salts added to the soils in the manure to those recovered by leaching, all vouched for by full analytical data.

Finally, King shows the effects upon the plants of different doses of manure, with respect to the water-soluble salts recoverable from the plants themselves. In both cases the influence of manuring is mainly seen to be a direct one, as has, in fact, already been shown by Godlewski. "It is thus shown that the crops on the manured ground have recovered 29 per cent. more potash from the four stronger soils, and 40 per cent. more from the poorer soils, where the fifteen tons of manure had been applied." Lime and magnesia, on the contrary, were diminished where the potash was increased.

What may be considered the final summing-up of this bulletin is given by King in the following paragraph on page 60, the last but one:

> The observations here presented, both upon the soils and upon the plants which had grown upon them make it clear that when farmyard manure is applied to fields it has the effect not only of increasing the yields, but at the same time of increasing the amounts of water-soluble salts which can be recovered from the soils themselves and from the plants which have grown upon them.

I have thought it necessary to present to the readers of SCIENCE somewhat in detail the contents of this bulletin E, in order to show what kind of work it is to which the bureau of soils refuses its *imprimatur*. To the unofficial mind—the *beschränkte Unterthanenverstand*—it appears as an admirable piece of work, in a line but little touched by agricultural investigators thus far, and manifestly likely to lead to important new lights, as well as to definite quantitative corroboration of old ones. As to bulletins D and F, respectively, on 'The Absorption of Water-soluble Salts by Different Soil Types' and on 'The Movement of Water-soluble Salts in Soils,' they are in a measure complementary to bulletin E, affording most interesting side-lights upon the general subject of the latter; they are altogether of similar high scientific grade. They also figure among the 'rejected papers.'

The clew to that rejection evidently lies in 'the published views of the Bureau of Soils,' which King for the time being does not desire to antagonize by discussion, as stated in the preface. What those views are is not specified; but it is easy to see that the results of King's work are wholly incompatible with the remarkable utterances of 'Bulletin 22,' now well known to all interested in agricultural science. Essentially, that bulletin promulgates the doctrine that while fertilizers 'do sometimes, and even frequently,' seem to increase production, yet since, according to data given therein, the aqueous soil solution is always of the same composition in all soils, it follows that all soils contain sufficient available plant food to maintain productiveness indefinitely; and that the moisture supply is the one controlling condition, climate permitting.

Such being the official, orthodox doctrine, it becomes clear why especially bulletin 'E,' showing pointedly the very reverse of the official doctrine to be true, could not receive the official approval and *imprimatur*. And that a man of King's standing and reputation could not, under such circumstances, do otherwise than tender his resignation, to take effect after his report had been completed and submitted, is obvious. This having been done, the Bureau of Soils is now rid of a contumacious, insubordinate person, who refuses to subscribe to his chief's scientific dicta as set forth in Bulletin 22; which, it is well known, has not received the assent of a single scientist of weight, and has been controverted and repudiated both in America and Europe by all who have taken any notice of it.

But worse than the ill-founded hypotheses of the head of one of the most important bureaus of the Department of Agriculture, which, moreover, receives and spends one of the largest appropriations in the budget of that department, is the return to medievalism indicated in the case before us. It is not only that of a deliberate attempt to suppress the truth, but it indicates on the part of the morally responsible head of that bureau a more than child-like confidence in the permanent success of the obscurantist régime such as is practiced and defended by Pobyedonostseff. Yet it is doubtful that even the latter, or the puissant head of the Russian Empire himself, would undertake to pass the censor's black brush over inductive scientific papers like these of King.

It is impossible to conceive that in the twentieth century, and especially in a country claiming to be progressive *par excellence,* such a régime should be allowed to continue for any length of time. King has uttered his *'e pur si muove'* by the publication of his rejected papers; it now behooves the scientific men of the country to voice their emphatic protest against the dictation of official orthodox science of any kind, from headquarters at Washington.

E. W. Hilgard.

Berkeley, Calif.,
September 29, 1904.

'THE METRIC FALLACY,' ONCE MORE.

To the Editor of Science: It is not uncommon for professors of linguistic science to be asked the question, 'What do you think will be the common language of the civilized world when the different peoples adopt one?' Despite the impossibility of direct knowledge on such a subject, conjecture is easy. Probably the most plausible of such conjectures is that the Teutonic and Romance languages will continue the present process of intermingling indefinitely until a common language becomes the result, difference of language diminishing into mere difference of dialect. It may be fair to assume that the English language, now the one most widely in use, will be the most important of the different components of the future language of civilization, though we have to admit the possibility that the Anglo-Saxon may give place within a few centuries to some progressive competitor, such as the Japanese. Supposing an international language thus to become developed by common consent due to common interests, international business both political and commercial will be facilitated. But even approximate uniformity of thought, of custom, of interest, has never yet received practical demonstration as a human possibility. If the future should develop a single universal language, it must be universal only in the sense of being a recognized standard from which many local offshoots will grow. No other view seems consistent with the continued existence of a reasonable degree of personal liberty.

Now, assume that a similar question is asked about the future coinage, weights and measures of the civilized world. The present chaos is bad enough, but far from being so bad as it was a century ago. The tendency has been unmistakably toward unification, but with the goal still far away. Any one who imagines that either the metric system or the British system, as formulated to-day, will meet all the requirements of both science and commerce a century or two hence, may be happy in his optimism, but he can not be credited with much appreciation of what experience has hitherto shown to be the processes of natural evolution.

In a recent communication (Science, September 16, p. 373) Mr. F. A. Halsey, writing in response to my criticisms of the attack upon the advocates of the metric system by Messrs. Halsey and Dale, says, " My purpose in writing this letter is to point out that Professor Stevens's admissions are of far greater importance than he seems to suspect." He considers me to have 'admitted pretty much all' that the antimetric contestants have contended for; but he admits that two important differences still exist. One is that I regard the change to the metric system as worth the cost, while he and Mr. Dale think it is not. The other is that I regard the change feasible, while he and Mr. Dale think it impossible. These two statements certainly indicate a considerable gulf between us, whatever may have

been the admissions made with a view to impartiality. The gulf seems incapable of being bridged by any structure built on the foundations set forth in Mr. Halsey's book. His self-complacent conclusion is, perhaps, the outcome of the fact that he is an avowed special advocate representing pecuniary interests that would suffer by the change considered, and that an advocate must claim everything in sight. His attitude can not possibly be judicial unless some miracle should suddenly bring about such a change of heart as to evoke words of penitence for his lack of appreciation of metric righteousness and of horror at his own British sinfulness.

What has been written by me about the metric system sets forth my own views alone. It was not intended to be representative of any other person or any pecuniary interests. It was an attempt to be fair, and as nearly as possible non-partisan, however cordial would be my welcome to the exclusive adoption of the metric system everywhere, if such were possible. There are doubtless many other metric advocates who do not share some of my views, and for whose opinions I entertain the highest respect. We may differ regarding the ease with which the change can be introduced, or as to the number of years of grace that should be allowed between initial and final legal enactments. The fact that my estimate regarding the limits of this period greatly exceeds that of some distinguished men does not in the least justify Mr. Halsey's assumption that I regard their view as 'worthless,' however limited may be the value of his own conclusion that the British inch is immovable, even 'until doomsday'—an infinity of years of grace. If my estimates are too liberal no one can hail the demonstration of such a mistake with more pleasure than myself.

My admission that among the uneducated on the continent of Europe the use of non-metric names and units is still common does not invalidate the claim of the metric advocates that the use of the metric system has become fairly well established among a majority of the educated classes in the same countries. Metric advocates who claim more than this are probably exceptional. They certainly would not reverse their opinions because the peasantry are now, have always been and can always be expected to be extremely conservative. Mr. Halsey devotes four fifths of the space in his book to a demonstration of the persistence of old units everywhere in spite of legislation. No such demonstration would be necessary for any reader who has paid reasonable attention to history, or who has had the opportunity to observe the uneducated in our own country. He gives a list, nine pages in length, of 'non-metric units used in metric countries,' with their American equivalents. M. Guillaume, of the International Bureau of Measures, has recently shown (*Physical Review*, September, 1904, pp. 234–237) that much of this table is worthless, not only containing information that is false, but quoting as non-metric the local names of units which are in value identical with metric units. Out of nearly five hundred entries in this table it is safe to say that a majority are local and almost unknown to international commerce. This grand parade of misinformation is sufficiently in accord with Mr. Halsey's assertion, 'We have the simplest and the most uniform system of weights and measures of any country in the world.'

It is quite possible for metric advocates to recognize the force of conservatism and vested interests, and yet to have faith in the future approach toward international unification of weights and measures. Even if this should be accomplished by legislation and sustained by public demand in all the great commercial centers of the world, the peasantry can be depended upon to hold on to their local inherited units and to furnish the data for such a table as Mr. Halsey has collected with so much care.

Mr. Halsey is disappointed that no special notice was taken of two chapters which he regarded the best in his book. He endeavors to make a sharp line of division between scientific men and manufacturers. Of the former he says: "They are measures, not makers, and their opinions have no value and no application as related to manufacturing." This estimate likewise is not surprising in view of

the elastic use of the word 'scientific' among the populace. The expression of contempt just quoted manifests a very limited horizon, and the 'measurers' are probably able to avoid intrusion. But the day for such sharp distinctions is past. Everything depends upon what we mean by 'scientific.' A man may devote years to chemistry, or physics, or biology, without learning scientific method, by making himself a measurer and resting satisfied with such routine. Mr. Halsey does not seem to remember that discovery is an aim in the pursuit of science, or that the demonstration of originality is incomparably more important in determining scientific standing than the ability to measure, however important this may be as a means. The mental training understood to be 'scientific' has to be applied in every pursuit where close observation, orderly arrangement, accurate calculation, independent thought, patient industry and good judgment are necessary. The recent presiding officer of the society representing English and American chemical industries, which convened a few weeks ago in New York, has a world-wide reputation as a scientific man rather than as a measurer. His successor as president is an American manufacturer, but one whose exceptional mastery of scientific method has enabled him to achieve distinction as a 'maker' of chemicals. According to Mr. Halsey's classification the opinions of Sir William Ramsay should be of no importance to Mr. Nichols; and if one of them is a metric advocate the other should be anti-metric. Our distinguished English guest has had many pleasant things to express about America, but in regard to our manufacturers he felt constrained to say: "The majority of them do not make it a practice to read scientific journals or to familiarize themselves with the latest trend of scientific thought. English manufacturers are far ahead of Americans in this respect, and Germany shows a clean pair of heels to both." What would he have said after reading Mr. Halsey's extraordinary views?

Any citizen of our country, who has an interest in educational, scientific or commercial progress has a right to an opinion, and to make himself heard if he can, regarding our weights and measures. To say that it is 'an industrial and commercial, and not a scientific question' is mere assumption. Some scientific manufacturers have expressed themselves in favor of the metric system. Mr. Halsey's claim, that scientific men who urge a change that may affect the immediate interests of manufacturers 'simply meddle with other people's affairs,' is as untenable as the claim of a politician who argues that no American has a right to urge tariff changes because they damage the interests of the trusts.

That the pecuniary interests to be affected by a change in our system of weights and measures are large and important is readily conceded. No one, moreover, will deny the great advantage of standardization for screw threads, pipes and textiles, or that a change of standards would be expensive. Mr. Halsey says that manufacturers know this to be 'impossible.' This is not conceded, either by metric advocates or by all manufacturers. In some departments of industry metric standardization has become established in Europe; and if for the same industry a metric standard is in use in one country and a British standard in another the abandonment of one of these will at last be necessary, even if it involves 'meddling with other people's affairs.' The two chapters of Mr. Halsey's book which he regards as the most important thus fail to establish anything more than what was already apprehended, that 'certain people would lose money and otherwise suffer much inconvenience by the change.' His assumption of 'impossibility' is entirely in keeping with the lack of 'scientific' or judicial fairness in the rest of his book.

The universal and exclusive employment of the metric system, if at all within the bounds of possibility, is so remote that it needs no consideration. What we do need, and what seems quite possible at no very distant day, is an international system of weights and measures, adopted by the central governments of the civilized world for use in all international commerce and in all government work. This would not compel American or English manufacturers to change their standards for

most of their customers, but it would put them to some extra expense and inconvenience if government purchases are made in the best market. Whether this international system should be exclusively metric is a matter of policy requiring careful consideration. The yard may be lengthened to equality with the meter, or the meter may be lengthened to forty inches, or many British units may be discarded and some metric units may be substituted for them, while some British units are retained. In any case the change, whatever it be, must tend toward unification and simplicity. It must necessarily cause initial increase of confusion, which will pass away without unreasonable delay in great commercial centers. Outside of such centers the people may be expected to hold on to their old habits; and in the remote rural districts hundreds of years may be insufficient to bring about uniformity. The essential desideratum is definiteness in value and simplicity in mutual relation among the units adopted. The mere nomenclature is of subordinate importance. Old names will certainly be retained by the masses even if values are modified, just as a dozen different values existed a few years ago for what was called foot, fuss, pied, etc. The change in values will be much easier for the masses if the old names are retained by legal provision; but this is a matter for which there is plenty of time. The Archimedean lever is indeed unknown, but even the English inch has been 'moved' in the past and Mr. Halsey's 'impossibilities' are no greater than what have been gradually overcome in the past and what may be gradually overcome in the future.

W. LE CONTE STEVENS.
WASHINGTON AND LEE UNIVERSITY,
October 3, 1904.

PROFESSOR WILLIAM MORTON WHEELER ON THE KELEP.

IN SCIENCE of September 30 (p. 437) Professor William Morton Wheeler has discussed the introduction into the United States of the kelep or Guatemalan cotton-protecting ant, and has reached decidedly adverse conclusions. Every new proposition must, of course, run the gauntlet of criticism, scientific and unscientific. Professor Wheeler claims special 'liberty to comment' because of 'exceptional opportunities,' but he nevertheless disregards several facts which might have mitigated the confidence of the prophecy.

It becomes apparent that the Poneridæ with which he is acquainted must be very different from the kelep. After observing colonies of *Ectatomma* and *Odontomachus,* both in nature and in captivity, I am ready to follow Mayr and Ashmead in assigning these genera to separate families, as unlike, indeed, as rats and rabbits. Whatever may be true of other Poneridæ or Odontomachidæ, it seems that the species of *Ectatomma* are widely distributed, enterprising ants. The kelep, instead of being a rare 'archaic' curiosity, is decidedly the dominant and most abundant insect of the Guatemalan cotton fields. The colonies, too, are several times as large as supposed by Professor Wheeler. They contain, usually, between 200 and 300 individuals, instead of from 40 to 50. There are seldom less than 100, and sometimes 400 or more.

The adaptability of the kelep is further shown by its association with the cotton for the sake of its nectar, as well as by its skill in stinging the boll-weevil. It is true, as Professor Wheeler says, that there are other pugnacious ants which 'attack' boll-weevils (or, for that matter, anything else which comes in their way), but they let them go again, and have no standing as 'destroyers.' To sting, disable, carry off, dismember and consume the pest, is still the unique distinction of the kelep.

Like some editors of newspapers Professor Wheeler will not be satisfied with the ants unless they absolutely exterminate the weevils, 'chase them into the Gulf of Mexico,' etc. The planters would probably be grateful, however for an addition of even ten per cent. to their crop—which illustrates the difference of standpoints. That the keleps make a regular practise of killing weevils renders them of distinct agricultural interest; the question is no longer whether they are useful, but whether we can get enough of them. Just how effi-

cient they might be made, even in Guatemala, is still unknown, for there the tree cottons are perennial sources of infestation, and all the climatic and cultural conditions favor the weevils. In Texas where the winter season will greatly assist in reducing the numbers of the beetles the utility of the ants may be correspondingly accentuated.

Of course it was highly improbable from *a priori* considerations that such an ant should be found, colonized in Texas, and made of practical use to the cotton industry. The chances are still very much against it, no doubt. It was obvious to Professor Wheeler from the first that the case was hopeless, but his warning has come too late, like the doctor whose patient had recovered. Plenty of difficulties remain, but the *post facto* prognosis is already in need of revision. The kelep is not being studied for the sake of results of 'negative, scientific value,' whatever that may mean, but because it appeared to the authorities of the Department of Agriculture that in so serious a difficulty every clue should be followed to some concrete conclusion. Unless the insurmountable obstacles can be pointed out more definitely it may be necessary to continue to seek them on experimental lines.

O. F. Cook.

Victoria, Texas,
October 4, 1904.

SPECIAL ARTICLES.

The Brain of a Swedish Statesman.

Retzius, in Vol. XI. of his extensive 'Biologische Untersuchungen,' presents a morphological study of the brain of a noted Swedish statesman. This is the fourth of a series of brains of eminent persons to be described by this investigator, the other three being those of the astronomer Gyldén, the mathematician Kovalevski, and the physicist and pedagog Siljeström. Retzius, in view of the rather negative results of older investigators in the field of cerebral morphology, and with the wish of satisfying himself whether the brains of persons of superior intellectual capacity were or were not to be distinguished from ordinary brains by special anatomical characters proposed, some time ago in conjunction with the physiologist Tigerstedt, that his colleagues in Stockholm bequeath their brains for scientific purposes. The forms of bequests received the signatures of just two men; viz., Retzius and Tigerstedt. Better results had been obtained by the Société mutuelle d'Autopsie (founded 1881) which now possesses ten brains or more, among them those of Gambetta, Bertillon, Vèron and G. de Mortillet. The Cornell Brain Association, founded by Professor Wilder in 1889, has bequeathed to it seventy brains of orderly educated persons, of which thirteen are already preserved in the Neurological Laboratory at Cornell. The American Anthropometric Society, formed in Philadelphia in 1890 possesses six brains of eminent scholars, and a number of others are promised. But such promises sometimes fail to be fulfilled, as happened in the case of Mrs. Elizabeth Cady Stanton. Her brain-bequest to Cornell, embodied in her will, was not read until after the funeral—designedly, it is stated, for her wishes on this head were well-known—because of the objections of relatives. It is indeed difficult, even under more favorable circumstances, after the death of a person eminent in human affairs to request—and obtain—the permission of the afflicted family for the removal, preservation and study of the brain.

In view of such adverse circumstances, Professor Retzius considers himself fortunate in having obtained, from time to time, the brain of some known person of superior mental power; and though the smallness of number of brains in this collection obviates the formulation of positive conclusions, he still deems it his duty to record descriptions of all brains of notable persons that might become available, before the problem of the interrelation of cerebral surface morphology and the mental aptitudes is entirely abandoned. The value of investigations of this kind has been repeatedly urged by Bischoff, Waldeyer, Manouvrier, Wilder and the writer ('Study of the Brain of the late Major J. W. Powell,' *American Anthropologist*, N. S., Vol. V., No. 4, 1903).

The identity of the Swedish statesman

whose brain Retzius now describes is not revealed in connection with this study, owing to the refusal of the sons of the deceased to accord permission to divulge the name. Retzius had, however, known him well since his youth and presents a few general remarks concerning the subject's intellectual capacity. The man showed great aptitude for learning early in life, was very successful in his studies at school and under the faculty of law. He rapidly advanced to the position of Minister of Finance (age 37) and three years after to that of Prime Minister. He was a provincial governor up to the time of his death at the age of 53. He is described as a highly gifted jurist, statesman, thinker, orator and philanthropist. Of large stature, dolichocephalic and of blond complexion, he belonged to the genuine Swedish type. His brain, removed on the second day after death by Dr. Curt Wallis, weighed 1489 grams. It was preserved in a mixture of 3 per cent. potassium bichromate and 2 per cent. formal, suspended in the fluid by a string tied to the basilar artery. The form of the brain was thus well preserved. As in all of Retzius' memoirs, this study is accompanied by beautifully executed photogravures and contour drawings.

The brain is well formed and richly convoluted. Viewed dorsally its shape is symmetrically ovoid with the greatest width in the subparietal region. The height is rather reduced. The association areas of the frontal and parietal regions exhibit a richness and complexity of fissuration, but there is hardly any noteworthy characteristic or redundancy of development in any particular territory. Nor were such findings to be expected. In life the man showed a well balanced intellect; his aptitudes were good in all directions, not in any special direction alone. Endowed with an excellent memory and good reasoning powers, he showed great skill and clearness of thought in parliamentary debate, without necessarily availing himself of purely rhetorical art. While not naturally devoted to any particular branch of the sciences, creative arts or human action, he could familiarize himself with all of these in the way of facile general understanding. This harmonius construction of the mental abilities is in no small measure correlative with that species of symmetry which this brain exhibited, and which is certainly exceptional in the richly convoluted brains of persons of highly developed but rather one-sided mental superiority. It may be noted, however, that the left subfrontal gyre ('Broca's gyrus,' the motor speech center) is somewhat favored in its development as compared with the same region on the right side.

EDW. ANTHONY SPITZKA.

SCIENTIFIC NOTES AND NEWS.

COLUMBIA UNIVERSITY has been celebrating the hundredth and fiftieth anniversary of its foundation as King's College during the past week. A large reception was given by the trustees on the afternoon of October 28. On the morning of October 31, the cornerstones of the School of Mines building, of a chapel, and of two dormitories were laid, and the physical training building of Teachers College was dedicated; in the afternoon a university convocation was held, and President Butler gave a commemorative address. Honorary degrees were conferred on thirty-three alumni, including Francis Delafield, M.D., 1863, emeritus professor of the practice of medicine in the College of Physicians and Surgeons, Columbia University; Edward Gamaliel Janeway, M.D., 1864, dean of the University and Bellevue Hospital Medical College; William Mecklenburg Polk, M.D., 1869, dean of the Cornell University Medical College; John Green Curtis, M.D., 1870, professor of physiology in Columbia University; William Henry Welch, M.D., 1875, professor of pathology in the Johns Hopkins University; Andrew James McCosh, M.D., 1880, visiting surgeon at the Presbyterian Hospital; Walter Kelnap James, M.D., 1883, professor of the practice of medicine in Columbia University and visiting physician at Roosevelt and Presbyterian Hospitals; William Bleecker Potter, A.B., 1866, E.M., 1869, mining and metallurgical engineer; Henry Smith Munroe, E.M., 1869, Ph.D., 1877, professor of mining in Columbia University; Frederick Remsen Hutton, A.B., 1873, E.M., C.E., 1876, Ph.D.,

1881, professor of mechanical engineering in Columbia University, and dean of the Faculty of Applied Science; William Stewart Halsted, M.D., 1877, professor of surgery in Johns Hopkins University, and surgeon-in-chief at the Johns Hopkins Hospital; Nathaniel Lord Britton, E.M., Ph.D., 1879, emeritus professor of botany in Columbia University, and director of the New York Botanical Garden; Moses Allen Starr, M.D., 1880, professor of neurology in Columbia University, consulting physician at the Presbyterian Hospital; Luther Emmett Holt, M.D., 1880, Carpentier professor of pediatrics in Columbia University, and attending physician at Babies' and New York Foundling Hospitals; Michael Idvorsky Pupin, A.B., 1883, professor of electro-mechanics in Columbia University; George Sumner Huntington, M.D., 1884, professor of anatomy in Columbia University; Ernst Joseph Lederle, Ph.B., 1886, Ph.D., 1895, former commissioner of health of the City of New York.

Dr. Albrecht Penck, professor of physical geography at the University of Vienna, who came to this country to attend the International Geographical Congress and the International Congress of Arts and Science, has been visiting Washington, Baltimore and New York, and is about to give a course of Lowell lectures in Boston.

Dr. L. Manouvrier, director of the anthropological laboratory of the École des Hautes Etudes and professor in the École d'Antropologie de Paris, gave an address before the Anthropology Club of Yale University on October 17.

The Journal of the American Medical Association states that the movement for a memorial to Dr. William Osler is taking the direction of a great medical and library building. It is thought that the collections will be national—possibly international—in extent.

Mr. Frank Springer, of Las Vegas, New Mexico, has been elected a foreign correspondent of the Geological Society of London, in recognition of his work on Crinoids.

Dr. Yersin has resigned the position of director of the Medical School of Hanoi, to become head of the Pasteur Institute of Nha Trang.

The British Secretary of State for the Colonies has authorized the conduct of a mineral survey of Nigeria, under the supervision of Professor Wyndham Dunstan, F.R.S., director of the Imperial Institute.

Nature states that Mr. H. Martin Leake, of Christ's College, Cambridge, has been appointed economic botanist to the government of the United Provinces, India, and proceeds at once to the botanic gardens, Saharanpur, N. W. P.

Dr. E. C. Richardson, of Princeton University, has been elected president of the American Library Association.

The annual address before the Geographical Society of Philadelphia was given on November 2 by the president of the society, Mr. Henry G. Bryant. His subject was 'Mexico and its People.'

Captain Robert F. Scott, of the British National Antarctic Expedition, will address the opening meeting of Royal Geographical Society.

Professor F. H. Getman, of Johns Hopkins University, is giving a course of sixty lectures on physical chemistry at the College of the City of New York.

Sir Charles Bruce made the introductory address at the opening session of the London School of Tropical Medicine on October 7.

Among those who will lecture in the public course at the College of the City of New York are Dr. William H. Maxwell, William Barclay Parsons and Dr. H. W. Wiley.

John Locke was born on August 29, 1632, and died on October 28, 1704. The two hundredth anniversary of his death was commemorated at the Johns Hopkins University on November 1. It was expected that addresses would be made by Principal C. Lloyd Morgan, of Bristol, England; Professor F. J. E. Woodbridge, Columbia University; Dr. William Osler, Johns Hopkins University; Professor J. McB. Sterrett, George Washington University, and Dr. William T. Harris, United States Commissioner of Education.

AT the sixth International Congress of Physiology at Brussels, Professor Heger, the president, inaugurated a movement to erect a monument to Marey, the inventor of the sphygmograph.

THERE will be a civil service examination on November 22 and 23 to fill positions as irrigation engineer and drainage engineer, in connection with the irrigation and drainage investigations in the Office of Experiment Stations, Department of Agriculture. The initial salary will be from $1,500 to $2,000 per annum according to qualifications. On the same days there will be an examination to fill the position of pharmacologist at a salary of $800, and of chemist and collector at a salary of $1,500 in the Bureau of Government Laboratories at Manila. On November 30 there will be an examination to fill the position of scientific assistant in the Bureau of Fisheries at a salary of $750.

THE International Society of the Directors of Museums met at Nuremberg, from October 3 to 5.

THE fifth Italian Pædiatric Congress will be held in Rome on October 28 to 31.

THE Committee on Awards of the Louisiana Purchase Exposition, St. Louis, have conferred upon the Wellcome Chemical Research Laboratories a grand prize and three gold medals, in recognition of the importance and educational value of the chemical and pharmacognostical researches conducted under the direction of Dr. Frederick B. Power.

A PARTY of about 150 French physicians and surgeons have paid a visit to London. They were entertained at the various medical institutions of the city.

AN amendment to the state constitution, which will be voted upon at the next election in November, provides for exempting from taxation the property of the California Academy of Sciences.

THE lectures given by Professor de Vries, at the University of California, during the summer of 1904, are being edited by Dr. D. T. MacDougal and will be published by the Open Court Publishing Co., of Chicago, in January, in a volume entitled 'Species and Varieties; their Origin by Mutation.'

WE learn from *The American Geologist* that the paleontological library of the late Professor Charles E. Beecher, of Yale University, is for sale. It comprises more than 3,000 pamphlets and 200 volumes. Those wishing for further information should apply to Professor Charles Schuchert, Yale University Museum, New Haven, Conn.

Nature states that an attempt is being made to establish an association of teachers of science, art and technology who are engaged in teaching at London institutions. It is hoped that the new association may become ultimately a national body. It has been agreed at meetings already held that the principal aims and objects of the association should be the general advancement of technical education; the interchange of ideas regarding methods of teaching technical subjects; the promotion and safeguarding of the professional interests of the members of the association in such matters as tenure, pensions and registration; to lay the views of the association before educational authorities and before the public; and to enable the members to cooperate as a body with other scientific and educational associations. Arrangements have been made for a general meeting to be held on October 22 at the Birkbeck College, Chancery Lane, at 3:30. All London teachers of science, art and technology, other than those employed in secondary schools, are invited to be present.

WE learn from *The British Medical Journal* that the next congress of French scientific societies will be held at Algiers in 1905. The following are the questions proposed for discussion in the section of medicine: Tuberculosis and the means of diminishing contagion; sanatoriums at high altitudes and by the seaside; hygiene of hot countries; methods of disinfection against contagious diseases and the results obtained in towns, in rural districts and in the institutions where the disinfection of dwellings and living rooms is practised; the conveyance of water to towns; the different forms of plague and its propagation; the part played by insects in the dissemination of disease; the prophylaxis of

malaria in Northern Africa; trypanosomiasis in Algeria.

The Geographical Journal states that a railway is projected from Turin to Martigny. The line would have a total length of 157 kilometers (97½ English miles), with a maximum gradient of one in twenty, while the traction would be electric, power for this purpose being supplied by waterfalls. Breaking off from the line connecting Modane with Turin, the new line would cross the plains of the Canavese to Pont, entering the valley of the Orco, and passing by a tunnel under the Paradis *massif;* then, mounting halfway up the left flank of the Aosta valley, it would pass Morge and Courmayeur, whence, by tunnel under the Col de Ferret, it would run in Swiss territory along the Dranse to Martigny. The new line, it is pointed out, would have the advantage over the Mount Cenis, St. Gothard, and Simplon lines as the shortest route between northwestern Italy and the countries of central Europe.

Los Angeles, Cal., has 3,000 acres of brush land called Griffith Park, which it intends to convert into a commercial forest. This is said to be the first instance of a city in the United States creating a forest. The practise is common in Europe, where the forest-parks have not only contributed to the pleasure of the people, but have been more than self-supporting through their timber output. Under its cooperative offer the Bureau of Forestry had last summer at Los Angeles four of its experts, making a comprehensive planting plan for the forest. This plan was completed at the end of September. The idea is to convert a waste piece of land into a productive forest, which will not only pay for its creation and care through the sale of mature timber, but will be a place of recreation for the citizens of Los Angeles.

UNIVERSITY AND EDUCATIONAL NEWS.

It is said that the Sheffield Scientific School, of Yale University, will receive $238,000 and land of unknown value under the will of the late Levi C. Viets.

Messrs. Palmer and Hornbostel, of New York, have, as the result of a competition, been appointed architects for the Carnegie Technical Schools at Pittsburg. It is said that $5,000,000 will be spent on them.

Ground has been broken for the new physics and biology building at Rochester University, which will cost $150,000.

At the twenty-first anniversary of University College, Cardiff, on October 14, it was stated that Lord Tredegar had promised £5,000 towards the cost of the new buildings shortly to be erected in Cathays Park; other sums were also promised amounting in all to £13,000. Between £70,000 and £80,000 are now available for the erection of the new buildings.

The registration of Harvard University is as follows: College, 2,002; Lawrence Scientific School, 524; graduate school, 358; divinity school, 42; law school, 731; medical school, 298; dental school, 109; making in all, 4,064; not counting Radcliffe College and the summer school. These figures represent a loss of about 200 students as compared with last year.

Registration figures at Cornell University show a total of 2,857, against a total last year of 2,656, a gain of 201. The distribution among the various colleges is: Arts, 648; law, 213; civil engineering, 377; Sibley, 1,040; agriculture, 178; veterinary, 104; medicine, 82; architecture, 68; graduates, 147. The greatest increase is 90 in Sibley College. These figures do not include the medical school in New York City.

Dr. F. S. Luther, since 1883 professor of mathematics at Trinity College, was installed as president of the institution on October 26. On the same day Dr. W. E. Huntington was installed as president of Boston University.

J. Culver Hartzell, Ph.D. (Munich), has been appointed professor of geology in the University of the Pacific.

Foster P. Boswell, Ph.D. (Harvard, 1904), has been appointed assistant in psychology; and Edwin Lee Norton, instructor in philosophy, in the University of Wisconsin.

Mr. H. M. Macdonald, F.R.S., has been appointed professor of mathematics in the University of Aberdeen.